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The Premise of Equipotentiality in Human Classical Conditioning: Conditioned Electrodermal Responses to Potentially Phobic Stimuli

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SUMMARY

The premise of equipotentiality, which has been widely adhered to among learning theorists, states that the laws of learning should not vary with the use of particular stimuli, responses, or reinforcements. This premise has recently been challenged by some data originating within the learning tradition itself, for example, studies on the effects of verbal stimuli in eye-lid conditioning. More importantly, however, the premise of equipotentiality is incompatible with data from experiments carried out within a biological–ethological framework. The results of such studies indicate that a given species is prepared to associate certain stimuli, responses, and reinforcers but not others. In an attempt to examine the validity of this premise in human classical conditioning, we investigated the effect of pictures of potentially phobic objects as conditioned stimuli (CSs) for electrodermal responses, since it has been suggested that phobias may be instances of biologically prepared learning. Three experiments are reported, all of them involving a long interstimulus interval differential conditioning paradigm with different pictures as CSs and electric shock as the unconditioned stimulus (UCS). In Experiment 1 we established that different pictures are differentially effective as CSs. A group conditioned to potentially phobic stimuli, snakes or spiders, showed greater resistance to extinction than a group conditioned to fear-irrelevant pictorial stimuli, that is, flowers or mushrooms. A third group conditioned to "representative laboratory stimuli," circles or triangles, fell in between these two groups. Experiment 2 showed that differences in salience did not produce similar effects to those observed with phobic and fear-irrelevant stimuli in Experiment 1. In Experiment 3 superior resistance to extinction for phobic stimuli was demonstrated when the UCS was an electric shock, but not when it was a tone to which the subject produced reaction times. Thus, the effect appears specific for aversive UCSs, and CS–UCS "belongingness" has been demonstrated. It was concluded that our data do challenge the premise of equipotentiality in human conditioning. There are several learning-theory accounts that could accommodate at least some aspects of the data, but they seem to be best explained in terms of biologically oriented constructs, such as preparedness.

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Within the framework of learning theory it has been more or less implicitly assumed that general laws of learning can be formulated without explicit reference to specific stimuli, responses, or reinforcements. Thus, it has been regarded as of little importance whether a law originates from experiments studying the associations between sounds and salivating in dogs, lights and eyeblinking in humans, or noise and "freezing" in rats. This assumption, which has been termed *the premise of equipotentiality,* has been widely held by learning theorists (for documenting quotations see Garcia, McGowan, & Green, 1972; Seligman & Hager, 1972; Shettleworth, 1972). During recent years, several lines of evidence have contributed to a reevaluation of this central assumption.

First, the premise of equipotentiality seems to be challenged by some data originating within the learning tradition itself. Investigating the effects of adding verbal communications to the conditioned stimulus (CS) in eyeblink conditioning, Grant and coworkers have accumulated evidence indicating that performance is a function of how the CS relates to the unconditioned stimulus (UCS) within a cognitive–verbal framework (see reviews by Grant, 1968, 1972). One example involves CSs that express response commands. In a differential classical conditioning paradigm it was found that superior differentiation between reinforced and unreinforced CSs (CS+ and CS−, respectively) was obtained when the command "blink" was the positive and "don't blink" the negative cue as compared to when the contingency was reversed (Bunde, Grant, & Frost, 1970).

Similarly, using positive and negative instrumental rewards of eyeblink responses, Fleming, Cerewicki, and Grant (1968) found better differentiation between "right" and "wrong" arithmetic problems when the former was positively and the latter negatively rewarded than when the reverse contingency was applied. These results, then, seem to indicate that a principle of "appropriateness" of the stimulus–reinforcement contingency or "belongingness" in the CS–UCS relationship is operating in eyeblink conditioning, at least when verbal CSs are used. Such principles obviously are at odds with the premise of equipotentiality.

The most compelling challenge to the premise of equipotentiality originates in studies being carried out within a biological, or ethological, framework (for reviews see Garcia et al., 1972; Hinde & Stevenson-Hinde, 1973; Rozin & Kalat, 1971; Seligman, 1970; Seligman & Hager, 1972; Shettleworth, 1972). Perhaps the most convincing refutation of the premise of equipotentiality was provided by Garcia and Koelling (1966). Using rats as subjects, they presented a CS compound of light, sound, and taste stimuli in conjunction either with x-radiation, which makes rats ill, or with electric shock as the UCS. When testing parts of the compound, they found superior conditioning to taste in rats who had received x-radiation and superior conditioning to sound or light in rats who had received electric shock. Thus, for rats, taste stimuli are easily associated with illness but not at all with shock, whereas the exteroceptive stimuli could be associated with shock-induced fear but not at all with illness.

These data, then, clearly indicate that a principle of belongingness between the CS and the UCS is operating in animal conditioning. Other results from taste-aversion studies also differ markedly from what would be expected from conventional learning theory. Thus, the responses are acquired on a single trial (e.g., Revusky & Bedarf, 1967), they are acquired over interstimulus

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intervals (ISIs) ranging in hours instead of seconds (e.g., Garcia, Ervin, & Koelling, 1966), they show very slow extinction (Mineka, Seligman, Hetrick, & Zuelzer, 1972), and they are not sensitive to informational characteristics in the situation (Kalat & Rozin, 1972). All of these results make eminent evolutionary biological sense, and consequently, taste-aversion learning has been used as a model phenomenon for the claim that learning must be viewed from an adaptive biological standpoint to be completely understood (Garcia, McGowan, & Green, 1972; Rozin & Kalat, 1971; Seligman, 1970; Shettleworth, 1972). Thus, while the data generated within the learning-theory tradition are possible to handle by elaboration of traditional theories (e.g., Grant, 1972), the data from the biological–ethological tradition have been interpreted as questioning the generality of learning principles and as emphasizing the need to study biological constraints on learning.

Given that biological constraints on learning are accepted on the empirical level, several approaches have been used on the theoretical level. These approaches could be ordered on a continuum where one endpoint is represented by theories assuming that a general associative mechanism supplemented by some new principles might account for the data, and the other by theories assuming highly specialized associative mechanisms depending on the specific properties of the events to be associated. The former approach may be illustrated by Revusky (1971) and by Testa (1974, 1975). They suggest that the laws of conditioning supplemented by principles of stimulus relevance and protection from interfering events (Revusky, 1971) or CS–UCS similarity in terms of stimulus location and temporal intensity patterns (Testa, 1974, 1975) are sufficient to explain taste-aversion learning.

Seligman (1970) has taken another and somewhat intermediate approach by suggesting that associations for a certain species can be ordered along a dimension of preparedness defined in terms of ease of learning. This dimension is founded on the assumption that evolutionary contingencies have prepared species to easily learn some things rather than others, thus implying that learning and instincts are points on a continuum. The dimension is operationally defined in terms of "how degraded the input can be before that output reliable occurs which means that learning has taken place" (Seligman & Hager, 1972, p. 4). So far the dimension is mainly a descriptive device for the ordering of learning tasks in terms of ease of learning. However, Seligman suggests that it may be used as a predictive theory if it can be shown that different parametric relationships of learning are valid for different regions of the continuum. Thus, assuming that it is possible to speak in general about "prepared learning," and taking taste aversion as a model phenomenon, it is suggested that prepared associations are very rapidly acquired and highly resistant to extinction, minimally affected by cognitive variables, and based on a physiological substate that differs from that of unprepared learning (Seligman & Hager, 1972).

The more extreme response to the obvious importance of biological factors in at least some instances of learning is to question the utility of general learning principles for any purpose other than the understanding of associations between arbitrary events in arbitrary laboratory situations. Such a position has been taken by Rozin and Kalat (1971) and Shettleworth (1972), who have argued that learning must be understood in terms of adaptive specializations closely fitted to the ecological demands of the different species. Therefore, they reject Seligman's (1970) idea that instances of biologically preprogrammed learning may show common characteristics.

The data implying the operation of biological constraints on learning and thus challenging the premise of equipotentiality mainly come from animal studies, and the paucity of human data in this field has become conspicuous. Indeed, a case has been made for the proposition that although general process-learning theory may be inadequate for the understanding of animal behavior, it may still be quite applicable to human behavior, since modern man in his man-made technological environment is typically required to learn about arbitrary events (Schwartz, 1974).

It is true that biological factors seem to be implied in human acquisition of language (e.g., Lenneberg, 1969) and in the notion of developmental stages (e.g., Flavell, 1963). In a massive extrapolation, Seligman (1970; Seligman & Hager, 1972) has suggested that such data may be incorporated in the preparedness theory. However, it would seem advantageous to obtain data challenging the premise of equipotentiality in a simpler conditioning paradigm that allows analysis of CS–UCS contingencies, and thus a more rigid experimental control of the phenomenon under interest, as well as a closer comparison with the animal data. A promising field for such an experimental attack might be human fears. Several authors have recognized the importance of biological–genetic factors in the acquisition of human fears and phobias (e.g., Lang, Rice, & Sternbach, 1972; Lazarus & Averill, 1972; Marks, 1969). For example, phobias are observed to a large but still limited set of potentially dangerous situations (e.g., Marks, 1969; Seligman, 1971). Thus, phobias for small animals or heights are quite frequent, whereas we seldom encounter people phobic for electric outlets, houses, or flowers. Similarly, phobias for small animals have been described since ancient times, and a phylogenetic origin seems to be suggested (Marks, 1969).

Since phobic disorders involve well-defined reactions of intense fear to specific situations or objects, learning theorists have been apt to interpret them as cases of conditioned responses (CRs). Such an analysis may be empirically supported by the famous demonstration of conditioned fear in little Albert (Watson & Raynor, 1920). However, this account of phobias seems to suffer from some rather severe limitations (e.g. Costello, 1970; Seligman, 1971). For example, whereas phobias are rapidly acquired, conditioned fear usually requires a number of trials to develop. Similarly, whereas phobias are very hard to extinguish, conditioned fear usually extinguishes quite readily. Since avoidance responses are very slow to extinguish, some authors (e.g., Eysenck & Rachman, 1965) have adopted an avoidance-conditioning theory of phobias. However, although the motor avoidance response is highly resistant to extinction, the conditioned fear that according to two-factor theory motivates the avoidance response extinguishes quite rapidly (see Seligman & Johnston, 1973), and presumably it is this component that is analogous to phobic fear. Further, by definition (Marks, 1969), phobias are irrational and noncognitive, whereas human-conditioned responses are very sensitive to cognitive factors (e.g., Grant, 1973; Grings, 1973). Finally, conventional conditioning theory is unable to explain the selectivity of phobias with regard to objects (Seligman, 1971).

Taking the selectivity of phobias with its implied biological background as a starting point, Seligman (1971) has suggested that the human species during its evolution may have become prepared to easily associate certain potentially dangerous situations with fear. Thus, he proposes that phobias may be regarded as instances of prepared learning. This interpretation not only accounts for the selectivity of phobias, but it also parsimoniously explains their very rapid acquisition, slow extinction, and resistance to rational arguments (Seligman, 1971).

Investigation of conditioning to potentially phobic stimuli, therefore, would seem to allow an opportunity to examine the validity of the premise of equipotentiality in human learning, as well as to shed some light on the acquisition of human fears and phobias.

In a recent series of studies, we have provided support for the theory that phobias are examples of prepared learning by demonstrating that pictures of potentially phobic objects are more effective CSs for human subjects than nominally "neutral," fear-irrelevant stimuli when paired with an aversive shock. In the first of these studies (Öhman, Erixon, & Löfberg, 1975), two groups of subjects were exposed to three classes of visual stimuli while skin conductance was measured: snakes, human faces, and houses. One group had shock paired with the phobic stimuli; in the other group, half of the subjects had shock paired with the faces and the other half with the houses. The results showed superior resistance to extinction in the first-interval anticipatory responses (FARs; cf. Prokasy & Kumpfer, 1973) in the subjects having the phobic stimuli paired with shock. There were no differences in second-interval anticipatory responses (SARs), but in third-interval omission responses (TORs) the phobic group tended to show better resistance to extinction than the neutral group. These results were followed up and extended in a second study (Öhman, Eriksson, & Olofsson, 1975) in which a single-cue paradigm was used. Thus, in contrast to the earlier study, each subject saw only one picture. The subjects having the phobic picture as the CS showed superior resistance to extinction in all response components compared to the subjects having fear-irrelevant pictures (houses) as the CS. Furthermore, the phobic group showed significant resistance to extinction after only one reinforcement, whereas the neutral group showed no significant resistance to extinction even after five reinforcements. Comparisons with groups given the CSs and the UCSs unpaired and groups given only the CSs revealed that the conditioning effect was a truly associative one.

Although these studies in general provide support for Seligman's (1971) preparedness theory of phobias, they are by no means decisive with regard to the premise of equipotentiality. Some limitations in the results must be pointed out. First, the effect of the phobic stimuli showed up clearly only in the resistance-to-extinction data. In the acquisition data only tendencies toward better conditioning were found. A second limitation pertains to the fact that the effect was not equally strong in all response components measured. Rather, it seems to be most clearly discernible in the FAR, less so in the TOR, and for the SAR no effect at all was observed in the first study.

Another, more important, shortcoming of these results with regard to challenging the premise of equipotentiality is that they do not permit a separation of "salience" factors in the CSs from a specific CS–UCS belongingness. Thus, the results so far might be explained in terms of the phobic stimuli being more salient than the fear-irrelevant, supposedly neutral stimuli. Such an interpretation is in line with several modern accounts of classical conditioning, which include CS salience as a factor that promotes "good" conditioning (e.g., Mackintosh, 1975; Rescorla & Wagner, 1972). From the preparedness standpoint, on the other hand, one would expect that the good conditioning to phobic stimuli would exclusively pertain to aversive UCSs. Such an outcome, furthermore, would provide a much more serious challenge to the premise of equipotentiality.

The present series of experiments were initiated specifically to evaluate *salience* versus *belongingness* as factors behind our previously reported results (Öhman, Eriksson, & Olofsson, 1975; Öhman, Erixon, & Löfberg, 1975). As a first step, it was necessary to assess whether the observed superior resistance to extinction of the phobic stimuli was due to comparatively good conditioning to phobic or "bad" conditioning to fear-irrelevant stimuli, since Öhman, Eriksson, and Olofsson (1975) noted that part of the success in demonstrating superior conditioning to the phobic stimuli could be attributed to the poor conditioning to fear-irrelevant stimuli. Since practically all human electrodermal conditioning studies have involved simple, artificial, and meaningless stimuli, such stimuli were used as a baseline condition for comparison with phobic and fear-irrelevant pictorial stimuli of the kinds previously used. Second, if the previous results were due to larger salience of the phobic stimuli, similar results should be generated by artificial stimuli differing in salience. This possibility was examined in Experiment 2. Finally, in Experiment 3, CS–UCS belongingness was studied by comparing phobic and fear-irrelevant stimuli as signals for an aversive UCS, with a condition in which these stimuli signaled a nonaversive UCS.

EXPERIMENT 1

In both the previous studies (Öhman, Eriksson, & Olofsson, 1975; Öhman, Erixon, & Löfberg, 1975), significant conditioning was observed during acquisition to the pictures of houses used as control fear-irrelevant stimuli. However, these responses extinguished immediately when the UCS was withheld. Since conditioned electrodermal responses to, for example, pure tones require several trials to extinguish (e.g., Öhman, 1974), this result led Öhman, Eriksson, and Olofsson (1975) to speculate that the fear-irrelevant stimuli they used might be sampled from the contraprepared side of the preparedness dimension postulated by Seligman (1970). This possibility makes it imperative to investigate whether the difference pertains to comparatively good conditioning to phobic or bad conditioning to fear-irrelevant pictorial stimuli by comparing both these conditions to more likely candidates for "unprepared" learning. The operational definition of the preparedness concept allows an assessment of *relative* preparedness in terms of a dependent variable, that is, criterion of learning, but it is doubtful whether our knowledge of evolutionary contingencies ever is specific enough to permit predictions from the independent-variable side of which stimuli are prepared, unprepared, and contraprepared with regard to a certain UCS. Therefore, these concepts cannot be used in an *absolute* sense. However, what is important here is to relate the previous findings with regard to the types of phobic and fear-irrelevant stimuli used to some more established neutral, normal, or unprepared stimuli.

In selecting potentially phobic stimuli, three criteria were used. First, the stimuli should involve typical phobic objects that are encountered among people in the clinic; second, a phylogenetic origin should be suggested; third, the stimuli should be easy to represent in pictorial form. On this basis, snakes and spiders were selected. The fear-irrelevant pictorial stimuli should (a) involve objects for which an evolutionary preparedness to be easily associated with shock-induced fear would be farfetched and (b) be similar to the phobic stimuli in general characteristics such as background. On this basis, flowers and mushrooms were selected because the pictures used showed the central object against a background of ground or grass as did the snake and spider pictures. Finally, the baseline "conventional laboratory stimuli" should be representative for the type of CSs generally used in electrodermal conditioning experiments. The basic requirement, therefore, was that these stimuli should be arbitrary, meaningless, and abstract. Thus, geometrical forms such as circles and triangles were chosen.

The primary purpose of Experiment 1 was to compare conditioning and resistance to extinction for the three classes of visual stimuli defined above. A second purpose was to see whether the effect of potentially phobic stimuli would be observable also in a genuine differential paradigm. Because such a paradigm allows unconfounded assessment of conditioning on a within-subjects basis (cf. Lockhart & Grings, 1963), it is likely to be a more sensitive tool than the previously used between-subjects design. In general, a differential paradigm is justified because snakes and spiders do not differ in orienting response (OR) elicitation (Öhman, Eriksson, Fredrikson, Hugdahl, & Olofsson, 1974). In the present design, which stimulus served as CS+ and CS− was counterbalanced so that, for instance, half of the subjects in the potentially phobic group had shock paired with the snakes and the other half with the spiders. Thus, this procedure also allowed an assessment of whether the results previously found with snakes might be extended to a second class of potentially phobic stimuli, that of spiders.

Method

Subjects. The subjects were 60 students attending introductory courses in psychology at the University of Uppsala. Half of them were paid, while participation of the other half was part of a course requirement. There were 38 females and 22 males, and the age range was between 20–28 years.

Apparatus. Skin conductance was measured by a Hagfors-type constant voltage circuit (Venables & Christie, 1973) through Beckman-Offner silver/silver chloride electrodes having a diameter of 8 mm and embraced in a plastic cup filled with Beckman-Offner electrode paste. Respiration, which was used as a control variable, was measured by a strain gauge fastened around the subject's chest. Skin conductance could be read from a digital display and was also recorded on paper on a Hewlett and Packard 7700 polygraph.

Electric shock UCS was delivered through silver electrodes from a capacitor that was charged by a manipulable, stabilized dc current.

Color slides, 24–36 mm, were projected from two Sawyer projectors on a milk-glass screen placed approximately 1.5 m in front of the subject. The size of the visible picture was 50 × 75 cm. The exposure time was controlled by two electronic timers. The interval between the successive exposure of the slide CS as well as the interval between the CS and the UCS had been programmed on a dual-channel Tandberg tape recorder, which started the timers by activating relay detectors.

The CSs consisted of three groups of color slides. There were 10 different slides each of snakes and spiders, triangles and circles, and flowers and mushrooms. The circles and triangles were black and appeared against brown or green backgrounds, the stimulus dimensions being perfectly correlated so that each subject discriminated between both form and color. The direction of the correlation, however, was counterbalanced. Each subject was shown two pictures, either snake–spider, circle–triangle, or flower–mushroom depending on which group he was allotted to. Thus, each subject saw the same two pictures throughout preacquisition-CS-alone presentations, acquisition, and extinction, but different subjects in the same group were shown different snakes, spiders, and so on, in order to randomize out influences from aspects of the pictures unrelated to the central themes. All pictures served as CS+ and CS− equally often.

Design. The basic design was a split-plot 3 × 2 factorial with the first factor (type of CSs) randomized and the second factor (CS+ vs. CS−) having repeated measurement. Thus, the design involved three groups of 20 subjects each. In the statistical evaluation of the data, a trial factor having repeated measurement was added. All conditioning effects, that is, CS+/CS− differences, were examined with a priori t tests. A rejection region of .05 was adopted for all comparisons.

Procedure. The subjects were allotted to groups according to a predetermined schedule. When reporting to the laboratory, each subject was asked to enter a dimly lit room and to sit down in a comfortable armchair. The apparatus was placed outside this room. Electrode sites were washed with distilled water and the skin-conductance electrodes were fastened by adhesive collars to the palmar side of the medial phalanx of the first and second fingers of the subject's left hand. The electric shock electrodes were placed on the tips of the first and second fingers of the subject's right hand. The respiration strain gauge was attached around the subject's chest.

The intensity of the shock UCS was determined individually for each subject. The experimenter gradually increased the shock level until the subject reported that the shock was "uncomfortable but not painful."

In order to facilitate differentiation and limit generalization across classes of stimuli, the subjects were explicitly informed about the CS–UCS contingency. Thus, each subject was informed that he would be shown two pictures and was told which picture would sometimes be followed by the shock UCS. Which stimulus served as CS+ and CS− was counterbalanced across subjects.

The duration of the CS pictures was 8 sec; shock followed immediately at the offset, producing an ISI of 8 sec. The intertrial interval (ITI) varied between 20 and 40 sec in steps of 5 sec with a mean of 30 sec. The pictures were presented in randomized orders with the restriction that the occurrence of more than two successive presentations of the same stimulus was prevented.

The preacquisition-CS-alone phase consisted of 8 CS-alone presentations, 4 to-be CS+, and 4 to-be CS−. During acquisition there were 5 reinforced presentations of CS+ and 5 unreinforced presentations of CS−. The extinction phase, finally, involved 10 unreinforced presentations of each of the two stimuli.

Response definitions. Skin-conductance multiple responses were measured in micromhos according to the criteria proposed by Lockhart (1966), and they were labeled according to suggestions by Prokasy and Kumpfer (1973). Thus, FARs were defined as the maximal change in conductance starting 1–4 sec after CS onset, and SARs were defined as the maximal deflection 4–9 sec after CS onset. During habituation and extinction, TORs were scored in the interval 1–4 sec after CS offset. Trials with respiratory irregularities were excluded from further analysis. Responses for all intervals during the entire experiment were range-corrected according to the suggestions by Lykken (Lykken, 1972; Lykken & Venables, 1971). Thus, each response was expressed as a proportion of the subject's maximal response during the experiment according to the formula $\Delta\phi_{ix} = \mathrm{SCR}_{ix}/\mathrm{SCR}_{imax}$. This implies that all the skin-conductance responses (SCR) of a certain subject (*ix*) were divided by his maximal response (*imax*) during the experiment. To justify this procedure, it is necessary to demonstrate that the groups do not differ in maximal responses.

Results

The groups did not differ in maximal responses, $F(2, 57) = .71$, the means being .729, 1.013, and .671 for the snake–spider, the circle–triangle, and the flower–mushroom groups, respectively. The results for the analyses of variance (ANOVAs) of the different response components from the different phases of Experiment 1 are summarized in Table 1, and the means for the three groups are plotted as a function of trials in Figures 1, 2, and 3.

Preacquisition-CS-alone trials. The FAR and TOR magnitudes decreased over trials,

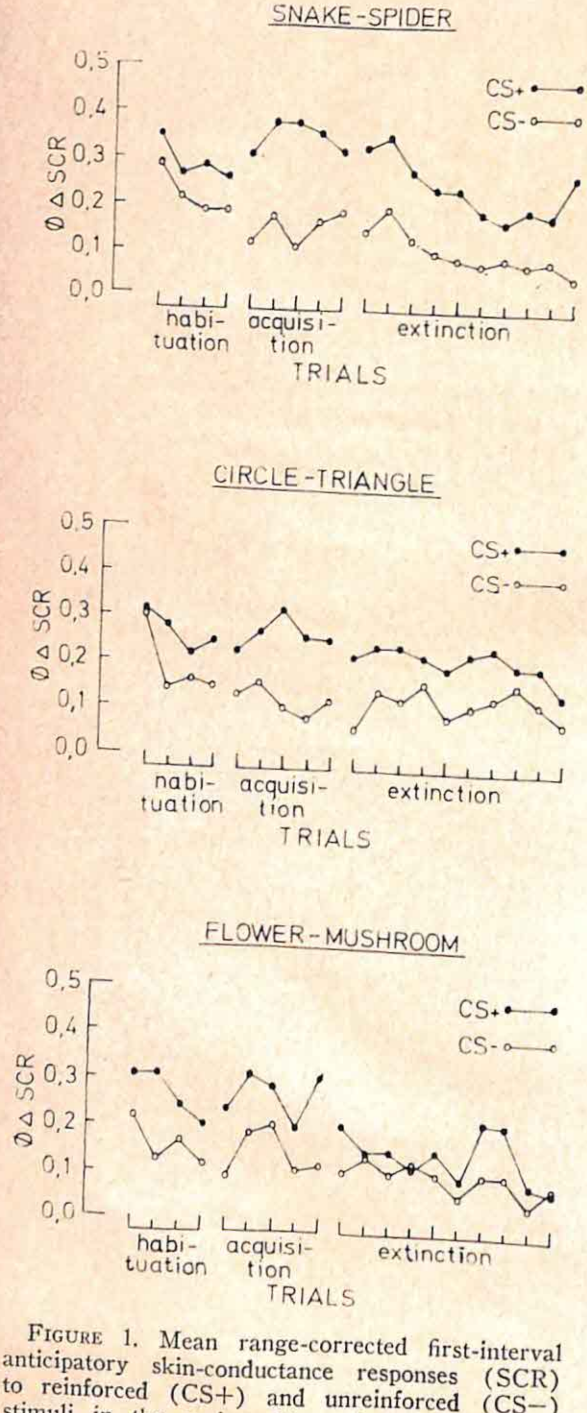

FIGURE 1. Mean range-corrected first-interval anticipatory skin-conductance responses (SCR) to reinforced (CS+) and unreinforced (CS−) stimuli in the snake–spider, circle–triangle, and flower–mushroom groups of Experiment 1 as a function of trials during habituation, acquisition, and extinction.

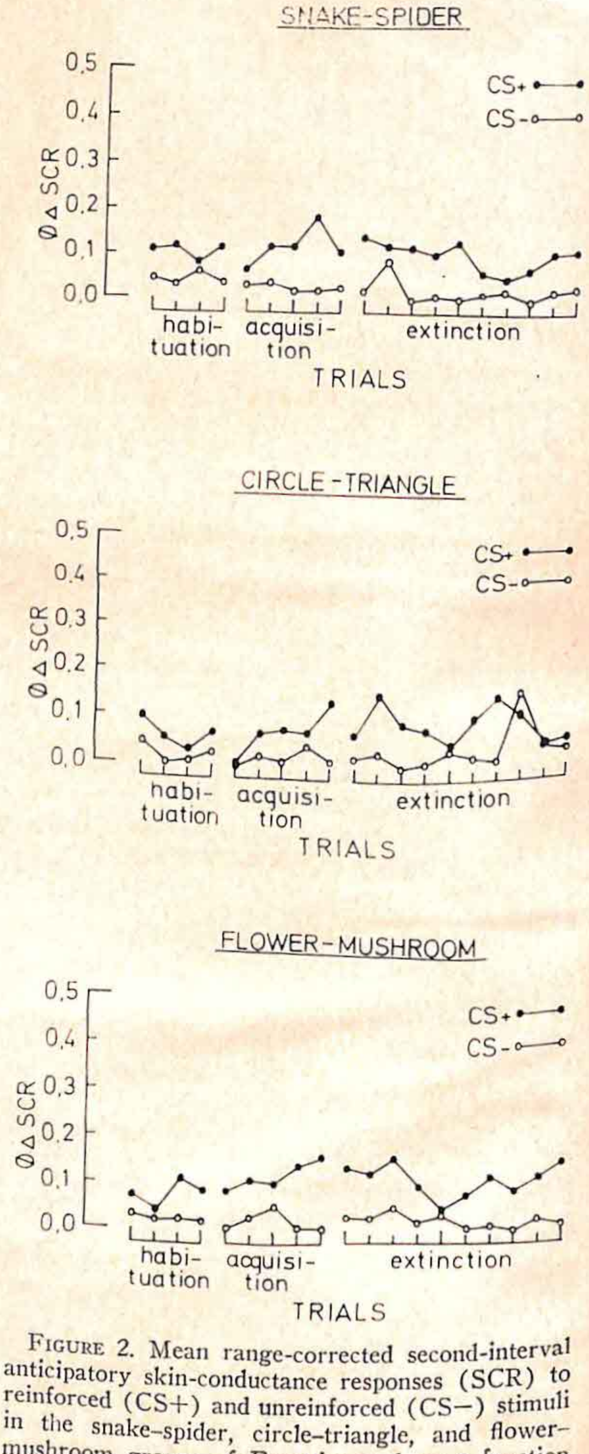

FIGURE 2. Mean range-corrected second-interval anticipatory skin-conductance responses (SCR) to reinforced (CS+) and unreinforced (CS−) stimuli in the snake–spider, circle–triangle, and flower–mushroom groups of Experiment 1 as a function of trials during habituation, acquisition, and extinction.

whereas the SAR magnitude remained essentially stable (see Table 1 and the left parts of Figures 1–3). The instruction produced differentiation between CS+ and CS− for all response components in the snake–spider, circle–triangle, and flower–mushroom groups, respectively: $t(57)=$ 1.96, 2.27, and 3.00 for the FAR; $t(57)=$ 2.77, 2.38, and 1.86 for the SAR; and $t(57)$ $=3.75$, 1.67, and 2.71 for the TOR. There were no differences in degree of differentiation between the three groups.

Acquisition. The acquisition curves for the FARs and the SARs are shown in the middle parts of Figures 1 and 2, and the F ratios are given in Table 1. There were highly significant overall effects of stimulus pairing on both response components, and the t tests indicated reliable conditioning in all groups. The t values in the snake–spider, circle–triangle, and flower–mushroom groups, respectively, were: $t(57)=$ 6.06, 4.64, and 4.23 for the FARs; and $t(57)=3.46$, 4.00, and 2.13 for the SARs. Additional evidence of conditioning was provided by the progressively better differentiation with training for the SAR as indicated by the significant CS+ Versus CS− × Trials interaction. All three groups appeared to acquire the responses at about the same rate, since the Groups × CS+ Versus CS− interaction failed to reach significance. However, when the differences between CS+ and CS− during acquisition were compared to the instructed differentiation during the preacquisition-CS-alone trials, only the snake–spider and circle–triangle groups increased the differentiation in FAR as a function of UCS administration, $t(57)$ $=3.33$ and 1.82, respectively, whereas the flower–mushroom group failed to do so. None of the groups increased the differentiation in SARs achieved by the instruction during the preacquisition-CS-alone trials when the UCS was introduced. Thus, there is some basis for suggesting that the flower–mushroom group showed less of a "genuine" conditioning effect than the two others.

Extinction. Extinction data are shown in the right parts of Figures 1, 2, and 3 and

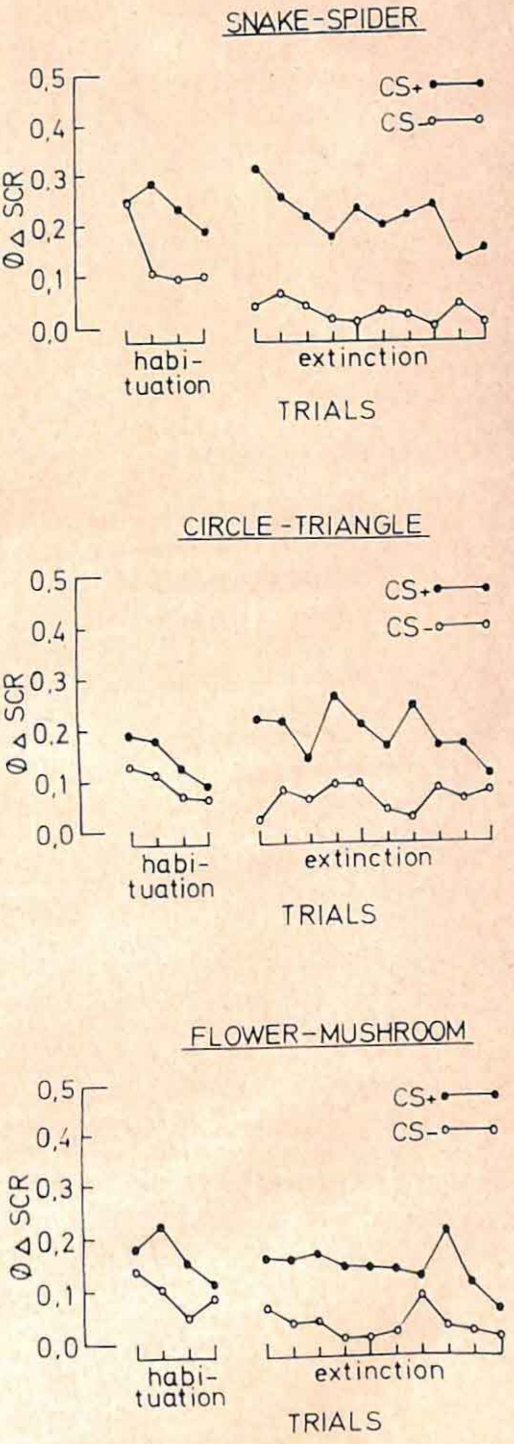

FIGURE 3. Mean range-corrected third-interval omission skin-conductance responses (SCR) to reinforced (CS+) and unreinforced (CS−) stimuli in the snake–spider, circle–triangle, and flower–mushroom groups of Experiment 1 as a function of trials during habituation and extinction.

TABLE 1

F RATIOS FOR THE VARIOUS RESPONSE COMPONENTS DURING THE DIFFERENT PHASES OF EXPERIMENT 1

Source	Habituation				Acquisition			Extinction			
	df	FAR	SAR	TOR	*df*	FAR	SAR	*df*	FAR	SAR	TOR
Groups (G)	2	1.40	2.41	2.10	2	<1	1.52	2	<1	<1	<1
Subjects within groups (SWG)	57	(.169)	(.033)	(.105)	57	(.220)	(.031)	57	(.366)	(.072)	(.200)
Stimuli (S)	1	17.93*	6.28*	23.04*	1	70.35*	30.57*	1	57.56*	42.52*	62.41*
S × G	2	2.96	<1	<1	2	1.26	<1	2	4.09*	<1	1.50
S × SWG	57	(.051)	(.021)	(.029)	57	(.055)	(.035)	57	(.049)	(.040)	(.096)
Trials (T)	3	12.67*	<1	10.15*	4	2.86*	3.15*	9	3.08*	1.26	2.14*
T × G	6	<1	<1	<1	8	<1	<1	18	1.52	1.58	<1
T × SWG	171	(.020)	(.012)	(.018)	228	(.028)	(.013)	513	(.032)	(.016)	(.024)
S × T	3	1.46	<1	3.64*	4	<1	2.84*	9	<1	<1	1.60
S × T × G	6	<1	<1	1.14	8	1.34	1.29	18	<1	1.33	1.12
S × T × SWG	171	(.018)	(.010)	(.013)	228	(.021)	(.013)	513	(.025)	(.016)	(.029)

Note. Error mean squares appear in parentheses; *df* = degrees of freedom; FAR = first-interval anticipatory response, SAR = second-interval anticipatory response, and TOR = third-interval omission response.
$* p < .05$.

the *F* ratios are given in Table 1.[1] Again, there was a highly significant overall differentiation between CS+ and CS− for all response components and for the snake-spider, circle–triangle, and flower–mushroom groups, respectively: $t(57) = 6.44$, 3.47, and 2.00 for FARs; $t(57) = 4.05$, 2.66, and 4.57 for SARs; and $t(57) = 5.93$, 4.23, and 3.55 for TORs. Both the FAR and the TOR decreased with trials but this effect appeared similar for both CS+ and CS−, since the Stimulus × Trials interactions were not significant.

The groups differed in resistance to extinction, however, as indicated by the significant interaction in FARs between differentiation and groups. In order to estimate these group differences, a mean difference score reflecting differentiation during extinction was calculated for each subject, and a one-way ANOVA was carried out on these values. The effect of groups was found to be significant, $F(2, 57) = 3.37$, and Tukey's honestly significant difference (HSD) showed a significant difference between the snake–spider group on the one hand and the flower–mushroom group on the other, $q(57) = .81$, indicating superior resistance to extinction in the former treatment. The circle–triangle group did not differ from the other two groups, which by conventional statistical logic implies that it occupied an intermediate position (cf. Winer, 1962).

As is obvious in Figure 2, the groups showed about equal resistance to extinction in SARs. For the TOR, however, there was a tendency toward better differentiation in the snake–spider group, but this effect did not reach significance.

Conditioning to individual stimuli. Half of the subjects in each group had shock paired with one of the cues and the other half had shock paired with the other cue. Thus, for instance, 10 subjects in the snake–spider group had snakes as CS+, and 10 subjects had snakes as CS−. By comparing these two subgroups in responding to snakes, the conditioning effect of this stimulus could be determined. Such between-subjects assessments of conditioning to each of the six different stimuli used in Experiment 1 were carried out with *t* tests on the extinction data so that it could be decided which of the individual stimuli were effective in bringing about resistance to extinction. The means and the *t* values for these comparisons are given in Table 2. For the snakes, there were significant resistances to extinction for the

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[1] For the purpose of comparison with other data, the means in μmho of conductance during extinction might be of interest. For the FAR CS+ and CS−, respectively, they were .461 and .189 for the snake-spider group, .346 and .177 for the circle–triangle group, and .230 and .168 for the flower–mushroom group.

SAR and the TOR but not for the FAR. For the spiders, all response components showed remaining stimulus-pairing effects. The flowers showed differentiation for SARs but not for FARs and TORs, and the mushrooms showed differentiation for SARs and TORs but not for FARs. The triangles failed to produce differentiation in FARs but did so for SARs and TORs, whereas there was no evidence of differentiation for the circles. Thus, phobic stimuli showed significant resistance to extinction in five out of six tests, the fear-irrelevant pictorial stimuli in three out of six, and the conventional laboratory stimuli in two out of six tests.

Discussion

Similar to previous findings (Öhman, Eriksson, & Olofsson, 1975; Öhman, Erixson, & Löfberg, 1975), the group conditioned to the potentially phobic stimuli showed reliably better FAR differentiation during extinction than the group conditioned to fear-irrelevant pictorial stimuli, such as flowers and mushrooms. The group given "representative laboratory stimuli," such as forms and colors, showed an extinction performance falling in between the two extremes represented by the snake–spider and flower–mushroom groups. Thus, the phobic stimuli tended to produce somewhat better and the fear-irrelevant pictorial stimuli somewhat worse resistance to extinction than the conventional laboratory stimuli. This picture is corroborated by the acquisition data and by the results from the tests of resistance to extinction of the individual stimuli. Thus, the snake–spider and circle–triangle groups, but not the flower–mushroom groups, increased the differentiation between CS+ and CS− achieved by the instruction when the UCS was presented during the acquisition phase, whereas the tests on individual stimuli essentially indicated that the phobic stimuli were superior to the two other stimulus categories.

It seems clear, therefore, that the previous results were due to comparatively good conditioning to potentially phobic stimuli, but that this effect may be enhanced by a tendency toward bad conditioning to fear-irrelevant pictorial stimuli. It might seem tempting to apply Seligman's (1970) definition of the preparedness dimension on these data and to regard the association between snakes/spiders and shock as relatively more prepared, and the associations between flowers/mushrooms and shock as relatively less prepared than the association between circles/triangles and shock. At any rate, the data show different categories of visual stimuli to differ in associability with the shock UCS, thus casting doubts on the validity of the premise of equipotentiality.

The fact that it is possible to demonstrate superior conditioning to phobic than to fear-irrelevant stimuli in a differential paradigm indicates that there is no extensive generalization across classes of potentially phobic stimuli. This is in agreement with clinical data, since phobias for small ani-

TABLE 2

MEAN RESPONSE MAGNITUDES TO REINFORCED (CS+) AND UNREINFORCED (CS−) CUES FOR STIMULI DURING EXTINCTION AND t VALUES FOR BETWEEN-SUBJECTS COMPARISONS IN EXPERIMENT 1

	FAR			SAR			TOR		
Stimulus	CS+	CS−	$t(18)$	CS+	CS−	$t(18)$	CS+	CS−	$t(18)$
Snake	.208	.084	1.54	.107	.015	2.03*	.203	.030	2.99*
Spider	.286	.124	2.70*	.092	.024	2.87*	.217	.023	4.18*
Flower	.136	.086	1.30	.109	.021	2.45*	.081	.036	1.69
Mushroom	.165	.109	.99	.111	.017	3.22*	.202	.027	3.04*
Triangle	.245	.156	1.12	.124	.039	2.67*	.226	.048	2.85*
Circle	.181	.078	1.61	.054	.033	.68	.154	.071	1.30

Note. Measured in range-corrected micromhos; FAR = first-interval anticipatory response, SAR = second-interval anticipatory response, and TOR = third-interval omission response.
* $p < .05$.

mals usually are monosymptomic (Marks, 1969).

From the present data, the spiders appear even more effective as CSs for electric shock UCSs than the snakes, since they were the only stimuli to show reliable resistance to extinction of all response components in the tests on individual stimuli. Thus, the previous results are not specific to snakes but may be extended to cover a second class of potentially phobic stimuli.

EXPERIMENT 2

The results from Experiment 1 show that potentially phobic stimuli show superior resistance to extinction to neutral stimuli. The nature of this effect, however, remains debatable. Thus, on the one hand, we may be dealing with preparedness in Seligman's (1970) sense, or on the other hand, the differences between groups might be explainable in terms of salience of the different stimuli. Thus, at least two recent theories of classical conditioning include a salience parameter that affects rate of learning (Mackintosh, 1975; Rescorla & Wagner, 1972).

If salience is operationally defined in terms of OR provocation, a salience theory could be elaborated and indirectly supported by data. Let us thus first assume that a highly salient stimulus provokes a large and slowly habituating OR. It might then be suggested that slow habituation of the OR is related to good electrodermal conditioning and, particularly, to superior resistance to extinction. This suggestion is supported by the findings of Öhman and Bohlin (1973), who compared groups of subjects showing slow and fast habituation of the electrodermal OR. The former group tended to show better FAR differentiation during acquisition than the latter, and more important, it was markedly superior in FAR differentiation during extinction. These results, then, are quite similar to those obtained with phobic and fear-irrelevant pictorial stimuli in Experiment 1. The third proposition in this alternative salience theory is that phobic stimuli give slower habituation of the OR than neutral stimuli, and this statement is strongly supported by data presented by Öhman et al. (1974). This theory, then, would account for the data on phobic conditioning in terms of OR-defined stimulus salience.

Attractive as this account may seem, there is still a crucial bit of evidence lacking. Öhman and Bohlin (1973) manipulated OR habituation by selecting subjects showing different rates of habituation, whereas OR habituation in the phobic case is manipulated by selecting stimuli that differ in salience. What is needed, therefore, is an experiment comparing conditioning and resistance to extinction of stimuli differing in salience and thus in OR habituation, but where the difference in OR elicitation between the stimuli cannot be regarded as due to factors like preparedness (cf. Klimova, 1965).

Berlyne and coworkers (Berlyne, 1958; Berlyne, Craw, Salapatek, & Lewis, 1963) have demonstrated that perceptually complex stimuli elicit more orienting than simple stimuli. Accordingly, in Experiment 2 we compared the performance of a group required to differentiate between highly discriminable but complex stimuli with a group differentiating between simple stimuli. If the former group shows slower habituation of the OR, and superior conditioning and resistance to extinction to the latter, our salience theory for the effect of phobic stimuli is strengthened. On the other hand, if the complex group shows slower habituation but fails to exhibit better conditioning and resistance to extinction compared to the simple group, then the OR and thus salience may be excluded as a factor mediating the effect of phobic stimuli.

Method

Subjects. Forty-seven subjects were recruited from introductory psychology course at the University of Uppsala. Twenty-two were paid, and 25 participated as a class requirement. There were 29 females and 18 males, and the age range was 19–27 years.

Procedure. The apparatus and the general procedure were identical to those used in Experiment 1. The CSs were chosen to differ in several of the complexity dimensions used by Berlyne (1958). Thus, the high-OR stimuli differed from the low-OR stimuli in irregularity of composition, number of elements, heterogeneity of elements, and irregularity of form. As high-OR stimuli, an abstract drawing was projected on a green or yellow background, the background thus providing cues for CS+ and CS−. The low-OR stimuli simply consisted of the background colors with no superimposed form.

To make sure that two types of stimuli did differ in OR provocation, one group of 15 subjects was exposed to 16 presentations of complex and simple stimuli in randomized order, so that response magnitude and habituation could be assessed to each of them (cf. Öhman et. al., 1974, Experiment 2).

In the main experiment, 16 subjects were exposed to the high-OR stimuli and 16 others to the low-OR stimuli. There was an initial habituation phase consisting of three exposures of each of the two colors serving as to-be CS+ and CS−. This was immediately followed by an acquisition session of eight reinforced presentations of one color and eight unreinforced presentations of the other, the cue serving as CS+ being counterbalanced across subjects. The terminating extinction series also involved eight presentations of each of the two cues.

The CSs were exposed for 8 sec with a shock UCS following immediately at CS offset during acquisition. The ITI varied between 30 and 50 sec with a mean of 40 sec. The subjects were *not* instructed about stimulus contingencies before the experiment. The responses were range-corrected as in Experiment 1.

Results

Habituation group. Six of the 15 subjects in this group failed to give a response to the first stimulus presentation, and therefore they were discarded. Because of this loss in degrees of freedom, the error terms were pooled (Winer, 1962, p. 202). There was an overall decrease in response magnitude over trials, $F(15, 248) = 5.15$. The responses were larger to the high-OR than to the low-OR stimuli, $F(1, 248) = 4.14$, the means being .129 and .079, respectively. The responses to the low-OR stimuli took fewer trials to reach a criterion of three successive zero responses than the high-OR stimuli, $t(8) = 2.07$, the means being 1.6 and 5.1 trials, respectively. Thus, it is clear that the high-OR stimuli did give more orienting than the low-OR stimuli.

Habituation in the main experiment. The maximal response did not differ between the high- and low-OR groups, $F(1, 30) = .24$, the means being .669 and .587 micromhos, respectively.

The responses were reliably larger to the high-OR than to the low-OR stimulus on the first stimulus presentation, $t(30) = 2.82$, the critical value being 2.44 with correction

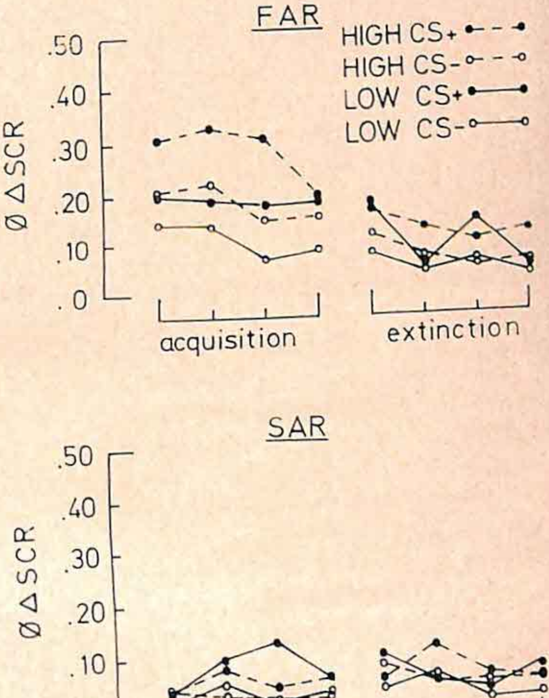

FIGURE 4. Mean range-corrected first- and second-interval anticipatory responses and third-interval omission responses (FARs, SARs, and TORs, respectively) to reinforced (CS+) and unreinforced (CS−) stimuli in the complex and simple groups of Experiment 2 as a function of trials during acquisition and extinction. (SCR = skin-conductance response.)

for pooled errors (Kirk, 1968, p. 268). This difference did not hold throughout the habituation phase, however, since the overall F did not reach significance, $F(1, 30) = 2.82$. Thus, as in the habituation study with phobic stimuli (Öhman et al., 1974), effects were more easily detected with a within-subjects than with a between-subjects design. The only significant effect during habituation pertained to a decrease over trials, $F(2, 60) = 20.96$.

Acquisition. The data from the main experiment are shown in Figure 4. Both the high- and the low-OR groups showed reliable differentiation in FARs, $t(30) = 3.03$ and 2.43, respectively, the overall effect being $F(1, 30) = 15.40$. The interaction between OR provocation and conditioning resulted in an F less than 1. The only other significant effect pertained to a decrease in magnitude over trials, $F(7, 210) = 2.95$.

For the SAR, the conditioning effect was reliable only in the low-OR group, $t(30) = 4.00$; for the high-OR condition, $t(30) = 1.62$. This difference between the two groups was not evident in the ANOVA, since here only the main effect of conditioning was significant, $F(1, 30) = 15.75$.

Extinction. The FAR and the TOR showed similar results during extinction. Thus, there was significant differentiation in both groups: $t(30) = 2.71$ and 2.57 for the FAR, and $t(30) = 2.56$ and 2.50 for the TOR, for the high- and low-OR groups, respectively.The overall effect was also significant: $F(1, 30) = 13.28$ and $F(1, 30) = 13.33$ for the FAR and the TOR, respectively. There was an overall decrease with trials for both measures, $F(7, 210) = 4.29$ for the FAR, and $F(7, 210) = 3.86$ for the TOR. No other effects were reliable.

For the SAR, there were larger responses to CS+ than to CS− in the low-OR group, $t(30) = 2.26$, but not in the high-OR group, $t(30) = .22$. The overall effect of CS+ versus CS− was not significant, $F(1, 30) = 3.25$, nor were any other effects.[2]

Discussion

The results from Experiment 2 show that a complex CS, in spite of producing more slowly habituating ORs than simple stimuli, fails to give more conditioning or better resistance to extinction than a simple CS. There is nowhere in the data even a tendency toward superior differentiation in the high-OR group. Thus, if it is accepted that salient stimuli give more orienting, it is highly unlikely that the effect of phobic stimuli found in Experiment 1 may be explained by the salience theory.

It could be argued that the difference in OR habituation between the phobic and neutral stimuli is larger than the difference between the complex and simple stimuli used in Experiment 2, and that, therefore, salience may still be a factor behind the conditioning results with phobic stimuli. However, the difference in trials to a criterion of habituation for the phobic and the neutral groups is of the same magnitude as that found between complex and simple stimuli in Experiment 2, that is, about three trials (cf. Öhman et al., 1974). Therefore, we may conclude that the results from Experiment 2 indicate that salience is not a factor behind the superior resistance to extinction of phobic stimuli.

EXPERIMENT 3

In Experiment 2 we demonstrated that stimuli differing in salience did not give results similar to those of phobic and fear-irrelevant pictorial stimuli in Experiment 1, and therefore, that salience is an unlikely explanation of the superior conditioning to phobic stimuli. In Experiment 3, the analysis of the phobic conditioning effect was pursued a step further by investigating whether it was possible to demonstrate belongingness between phobic stimuli and aversive UCSs. Thus we examined whether the superior resistance to extinction of phobic stimuli was specific for the electric shock UCS by comparing phobic and fear-irrelevant pictorial stimuli as signals for such an event with a condition where these stimuli signaled a tone to which the subject was required

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[2] The means for the FAR in μmho during extinction were .248 and .118 for the CS+ and the CS− in the high-OR group, and .227 and .123 in the low-OR group.

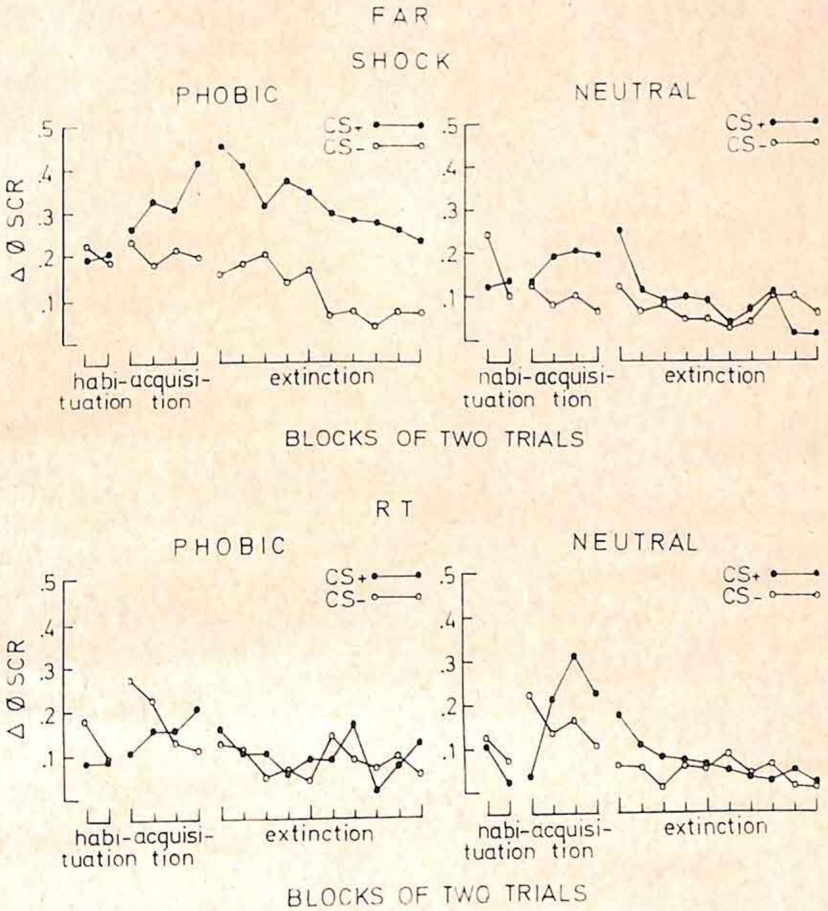

FIGURE 5. Mean range-corrected first-interval anticipatory responses (FARs) to reinforced (CS+) and unreinforced (CS−) stimuli in the phobic–shock, neutral-shock, phobic–reaction time (RT), and neutral–RT groups of Experiment 3 as a function of trials during habituation, acquisition and extinction. (SCR = skin-conductance response.)

to produce reaction times by pressing a key. It is clear that conditioninglike effects are obtained to the warning stimuli in reaction time tasks; that is, there is more electrodermal activity to a stimulus signaling the reaction time task than to a similar stimulus presented alone (Baer & Fuhrer, 1969; Fuhrer & Baer, 1970).

It should be noted that a demonstration of CS–UCS belongingness excludes the possibility of any general property of the CS being the basis for superior conditioning, since it shows that the effect is due to *associations* between events (cf. Schwartz, 1974). Furthermore, while belongingness is directly predicted by the biologically oriented theorists (e.g., Rozin & Kalat, 1971; Seligman, 1970), it is difficult to handle within a learning-theory framework.

Method

Subjects. Forty psychology students volunteered as subjects. There were 30 females and 10 males, and the age range was 20–30 years.

Procedure. The apparatus and general procedure were identical to those in Experiment 1, with two exceptions. First, the subjects were placed in a specially constructed sound-deadening experimental chamber, which allowed better control of extraneous variables and also resulted in larger CS pictures, 60 × 95 cm, than the one used in Experiment 1. Second, *no instructions* about stimulus contingencies were given prior to the experiment.

There were four experimental groups arranged in a 2 × 2 factorial design with phobic (snakes and spiders) versus neutral (flowers and mushrooms) stimuli and shock versus reaction time tasks as factors. A differential paradigm similar to that in Experiment 1 was used. During the habituation phase, 4 presentations each of the to-be CS+ and CS− were given. Acquisition involved 8 reinforced CS+ presentations and 8 unreinforced presentations of CS−. The extinction phase, finally, was extended to 20 presentations of each of the two cues.

The CS pictures were exposed for 8 sec; the shock UCS, which was identically defined to that in Experiment 1, followed immediately at offset. The imperative stimulus for the reaction time task was a tone that also followed immediately at CS offset during acquisition. The tone had an intensity of 84 dB (A) and was turned off when the subject pressed the response key that was placed in his right hand. The ITI varied between 20 and 40 sec with a mean of 30 sec.

Results

The groups did not differ in maximal responses: $F(1, 36) = 1.50$ for the phobic–neutral comparison and $F(1, 36) = .76$ for the UCS comparison, Thus, the range correction is justified.

Habituation. Only the FAR was analyzed during habituation, since no instruction about stimulus contingencies was given in Experiment 3. The phobic groups did not differ from the neutral ones, $F(1, 36) = 1.49$ (see Figure 5). The groups exposed to the shock UCS in the workup procedure were sensitized to show larger responses than those in the reaction time (RT) groups, $F(1, 36) = 6.32$. The responses to the to-be CS− were larger than those to the to-be CS+, $F(1, 36) = 5.66$, and the former responses decreased more in magnitude than the latter, $F(1, 36) = 8.32$, for the Trials × Conditioning interaction. These effects were due to the fact that the first stimulus always was a CS−.

Acquisition. The F ratios for the ANOVAs excluding the sources involving trials are given in Table 3. For the FAR, there was reliable acquisition only in the shock groups: $t(36) = 3.71$ for the phobic and $t(36) = 2.85$ for the neutral group. All four groups showed diverging trends for CS+ and CS−, however, which may be construed as evidence for some acquisition also in the RT groups, $F(3, 108) = 21.20$, for the Trials × Conditioning interaction. In order to maximize the possibilities of observing acquisition effects on the FAR in the RT groups, the first extinction trial was taken as a test trial (cf. Öhman, Eriksson, & Olofsson, 1975). Reliable differentiation between CS+ and CS− occurred in the neutral group, $t(18) = 1.90$, but not in the phobic group, $t(18) = .66$.

The means for the SAR are shown in Figure 6, and the F ratios for the stimuli, UCS, and conditioning factors are given in Table 3. The responses were larger in the shock than in the RT groups. The application of reinforcements resulted in reliable overall conditioning. Individual t tests, however, indicated reliable differentiation only in the phobic–shock group, $t(36) = 2.31$, and neutral–shock group, $t(36) = 2.80$, although the UCS × Conditioning interaction failed to reach significance.

TABLE 3

F RATIOS FOR THE VARIOUS RESPONSE COMPONENTS EXCLUDING THE SOURCES INVOLVING TRIALS DURING THE DIFFERENT PHASES OF EXPERIMENT 3

	Acquisition			Extinction			
	df	FAR	SAR	*df*	FAR	SAR	TOR
Groups (G)	1	2.29	1.61	1	5.83*	6.20*	10.32*
Unconditioned stimulus (UCS)	1	<1	4.39*	1	4.32*	1.30	1.99
G × UCS	1	2.85	1.01	1	2.21	1.43	1.29
Subjects within groups (SWG)	36	(.1438)	(.0357)	36	(.2522)	(.0841)	(.0925)
Stimuli (S)	1	9.41*	7.66*	1	20.90*	4.26*	25.64*
S × G	1	<1	<1	1	10.29*	<1	6.71*
S × UCS	1	7.75*	3.27	1	10.61*	3.13	10.25*
S × G × UCS	1	1.90	<1	1	12.41*	1.31	9.58*
S × SWG	36	(.0228)	(.0087)	36	(.0411)	(.0122)	(.0110)

Note. Error mean squares appear in parentheses; *df* = degrees of freedom; FAR = first-interval anticipatory response, SAR = second-interval anticipatory response, and TOR = third-interval omission response.
$* p < .05$.

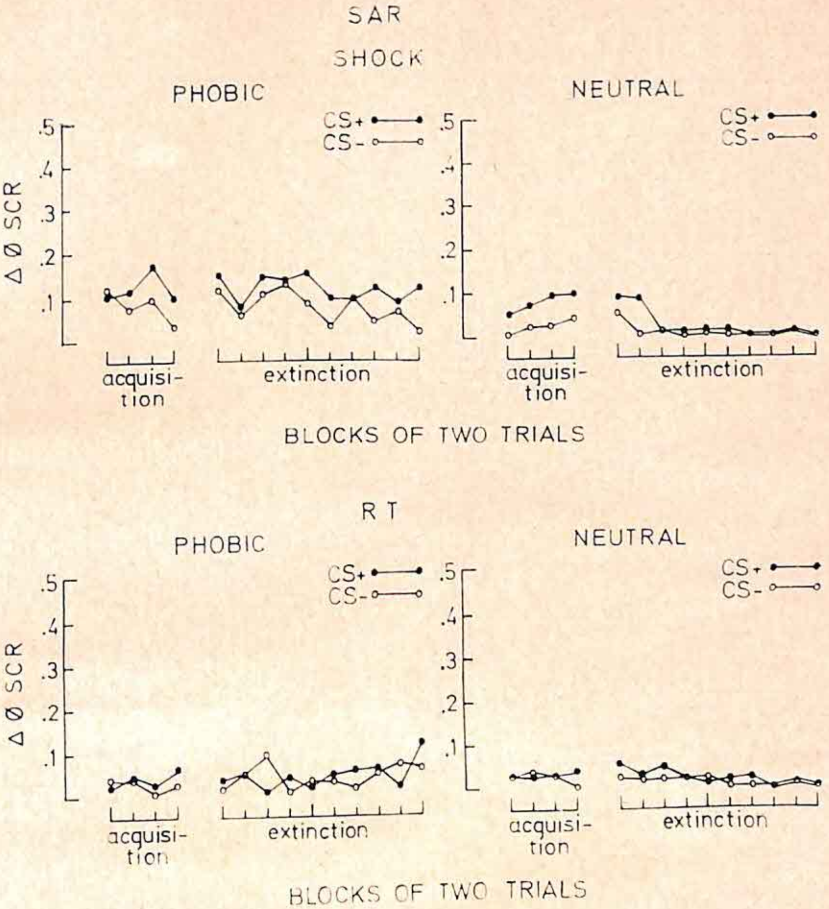

FIGURE 6. Mean range-corrected second-interval anticipatory responses (SARs) to reinforced (CS+) and unreinforced (CS−) stimuli in the phobic–shock, neutral-shock, phobic–reaction time (RT), and neutral–RT groups of Experiment 3 as a function of trials during acquisition and extinction. (SCR = skin-conductance response.)

Extinction. FAR magnitude during extinction is shown in Figure 5 and the *F* ratios are given in Table 3.[8] The outstanding feature of this set of data was the marked elevation in response magnitude to CS+ in the phobic–shock group. In fact, this effect was so strong that it resulted in statistically significant effects in all comparisons it entered. Thus, the phobic-group means exceeded the neutral-group means, and the shock-group means exceeded the RT-group means. The responses were larger to CS+ than to CS−, and all interactions except those involving the trial factor were reliable; *t* tests revealed lasting effects of stimulus pairing only in the phobic–shock group, $t(36) = 7.25$. There was an overall decrease in magnitude during the extinction trials, $F(9, 324) = 5.37$. This overall decrease is most likely due to dissipation of the sensitization effects produced by the UCS during acquisition, since the trends for CS+ and CS− did not converge. To further elucidate this aspect of the data, a separate ANOVA on the phobic–shock group was carried out, where the two first trial blocks were compared to the two last trial blocks for CS+ and CS−. The interaction between conditioning and trial blocks, however, failed to reach significance, $F(1, 9) = .52$, which in-

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[8] The FAR means to CS+ and CS− in μmho during extinction were .923 and .314 for the phobic–shock group, .165 and .145 for the neutral–shock group, .136 and .095 for the phobic-RT group, and .108 and .070 for the neutral-RT group.

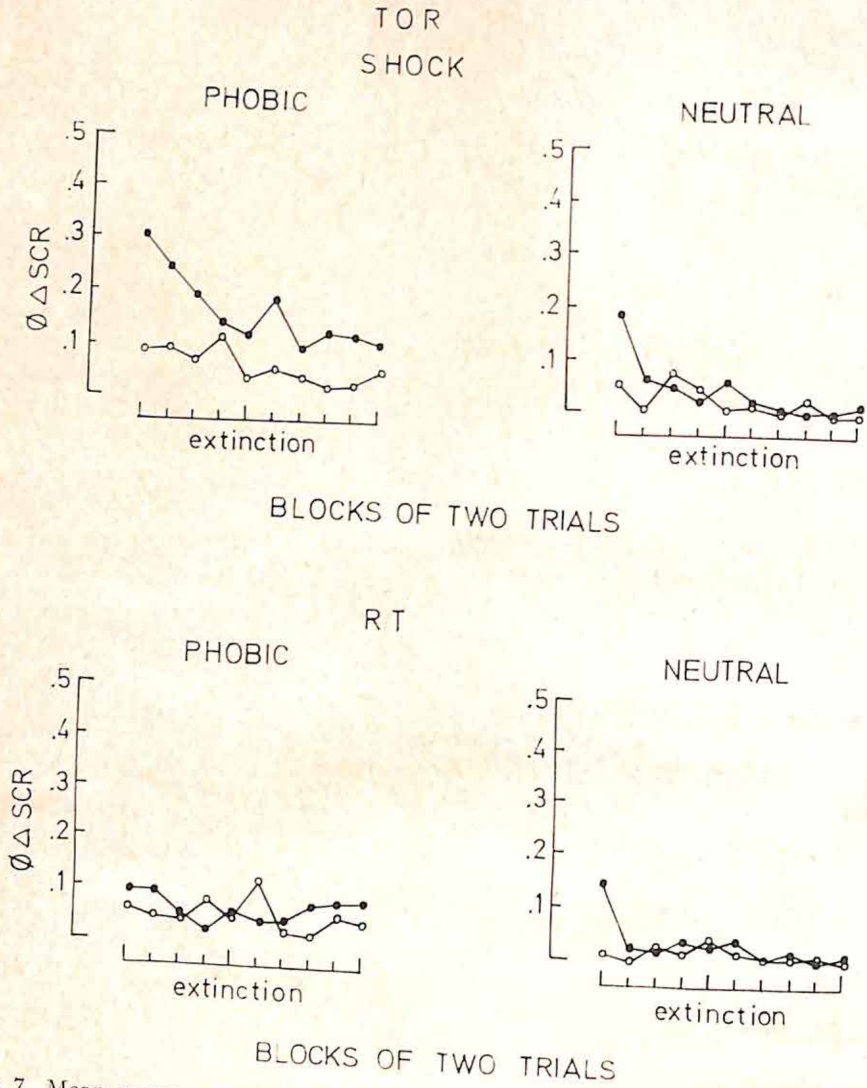

FIGURE 7. Mean range-corrected third-interval omission responses (TORs) to reinforced (CS+) and unreinforced (CS−) stimuli in the phobic-shock, neutral-shock, phobic-reaction time (RT), and neutral-RT groups of Experiment 3 as a function of trials during extinction. (SCR = skin-conductance response.)

dicates that no measurable extinction effect was observed during 20 unreinforced CS presentations.

The SAR (see Figure 6 and Table 3) was larger to the phobic than to the neutral stimuli, and overall effects of stimulus pairing were observed. However, *t* tests on the individual groups failed to reveal differentiation in any of the four groups.

Figure 7 shows the data for the TOR during extinction, and the *F* ratios are given in Table 3. The results for this measure were very similar to those of the FAR. Thus, there was a highly reliable differentiation between CS+ and CS− in the phobic-shock group, $t(36) = 7.03$, but not in the other groups (these statistics are completely consistent with those from the ANOVA). However, the trends for CS+ and CS− converged as a function of trials, $F(9, 324) = 3.70$, thus providing evidence for overall extinction effects. An ANOVA similar to that performed on the FAR in the phobic-shock group, however, failed to indicate a tendency toward extinction of the TOR in the phobic-shock groups, since the interaction between conditioning and trials did not approach significance, $F(1, 9) = 1.62$.

Skin-conductance level (SCL). It could be suspected that the shock condition produced more unspecific activation than the RT conditions, which in turn might affect the differences found in phasic responses. To check this possibility, SCL was scored immediately before every fourth trial, and then Measurement Points × Phobic/Neutral × Shock/RT ANOVAs were carried out for each phase of the experiment. However, no significant effects were found, although the means tended to be somewhat higher for the shock than for the RT groups, for example, 17.42 and 14.73 micromhos for the shock and RT groups, respectively, during extinction.

Discussion

The results from Experiment 3 again demonstrate quite convincingly that potentially phobic stimuli are more effective CSs than fear-irrelevant neutral stimuli and, furthermore, that this effect is obtained only with the aversive UCS. Thus, CS–UCS belongingness was demonstrated. Similar to the previous studies, the results were most clear-cut for resistance to extinction and for the FAR and the TOR components.

A potential difficulty in interpreting these findings arises because of the weak differentiation found between the CS+ and the CS− in the RT groups. Thus, it could be argued that a RT task is ineffective as a UCS regardless of which CSs are used. There are two factors that militate against an observation of stimulus-pairing effects in the RT groups of our study. First, differentiation was counteracted by the significant reverse differentiation during habituation. Inspection of Figure 5 reveals that in both RT groups the FAR to CS+ increased, whereas the FAR to CS− decreased as a function of training. This effect contributed to the significant Trials × Conditioning interaction, but because of the different starting point for the two curves, these diverging trends did not result in significant overall differentiation. However, when examined during the first extinction trial, significant differentiation occurred in the neutral group.

The second factor counteracting differentiation in the RT group involves the relatively few training trials used. Baer and Fuhrer (1969; Fuhrer & Baer, 1970) have clearly demonstrated that differentiation is obtained with RT as the UCS, given that a sufficient number of acquisition trials are presented. Thus, it can be asserted that there was evidence of acquisition in our data, but that these effects were masked by the reversed initial differentiation between CS+ and CS−, and by the relatively few training trials used. The important point, furthermore, is that acquisition can be obtained with an RT task as the UCS, and for this proposition we have good empirical evidence (Baer & Fuhrer, 1969; Fuhrer & Baer, 1970). Thus, a number of acquisition trials with a nonaversive UCS that ultimately will produce conditioning (the RT task) gave tendencies toward better conditioning to the fear-irrelevant than to the phobic stimuli, whereas the same number of acquisition trials gave very clear evidence of superior resistance to extinction to phobic stimuli when the UCS was an aversive electric shock. In this sense, then, belongingness was demonstrated.

CONCLUDING DISCUSSION

The results from Experiments 1 and 3 clearly substantiate previous findings (Öhman, Eriksson, & Olofsson, 1975; Öhman, Erixon, & Löfberg, 1975) that potentially phobic stimuli, which have been paired with an aversive electric shock, give superior resistance to extinction of conditioned electrodermal responses. The results from the circle–triangle group in Experiment 1 seem to indicate that there is no dichotomy between phobic and fear-irrelevant pictorial stimuli, but that we may be dealing with a continuous dimension of CS effectiveness in relation to a certain UCS (cf. Seligman, 1970).

In Experiment 2 we demonstrated that salience of the CS is an unlikely basis for the difference between phobic and fear-irrelevant stimuli, since other stimuli differing in salience did not produce similar effects. The results of Experiment 3, finally, demonstrated that a principle of CS–UCS belongingness seems to be operating because the superior resistance to extinction of phobic stimuli was observed only when the UCS was an aversive electric shock. The data from the present series of experiments, therefore, provide strong doubts regarding the validity of the premise of equipotentiality for human conditioning.

The present results confirm that the effect of the phobic stimuli pertains particularly to a failure to extinguish. Indeed, in Experiment 3 we failed to observe even a tendency toward a decrease in FAR differentiation, even when extinction Trials 1–4 were compared with Trials 16–20.

Similar to previous findings, the effect of the phobic stimuli was most clearly discernible in the FAR, less so in the TOR, and not at all in the SAR. There are quite strong reasons to regard the FAR as involving orienting activity indicating processing of the CS (e.g., Öhman, 1971a; Öhman, 1972) and the SAR as involving expectancy-type cognitive activity (e.g. Fuhrer & Baer, 1969; Lockhart, 1973; Öhman, 1971b). It would appear, therefore, that the effect of phobic stimuli has to do with changes in the meaning of the CS rather than being related to expectancy of the UCS.

While the results seem reasonably clear and consistent, and while their inconsistency with the premise of equipotentiality appears quite compelling, the theoretical interpretation of the findings is less clear. The most basic question, of course, is whether some biological factor is needed in the explanation or whether the results may be accounted for solely in terms of learning. The primary advantage with a learning interpretation is that it involves less strong assumptions than a biological–genetic interpretation. An adequate account of either kind, however, must be sufficiently specific to reasonably account for the detailed empirical data we have obtained. In the following analysis we first examine some relevant learning theories, and then we turn to a consideration of the various biologically oriented alternatives.

The first theory to be considered is the Rescorla and Wagner (1972) theory of classical conditioning. This theory can handle variations in CS effectiveness due to salience, since it assumes that this factor is one of the parameters that determines rate of learning. As shown by the results of Experiment 2, however, salience is an insufficient explanation for the phobic-conditioning effect. Furthermore, since the CS salience and the UCS effectiveness are regarded as independent parameters, this theory cannot account for the belongingness demonstrated in Experiment 3.

Recently, Mackintosh (1975) has advanced an attention theory of conditioning, which formally is very similar to the Rescorla and Wagner (1972) theory. The major difference is that the salience parameter is assumed to reflect attention and that it changes with training. Thus, if a stimulus predicts reinforcement better than all other stimuli together, its salience will increase. If the reverse is true, it will decrease. These assumptions, then, may provide a mechanism for producing differences in salience between phobic and fear-irrelevant stimuli. So far, however, this theory suffers the same shortcomings as the Rescorla and Wagner theory in accounting for our data. An interesting feature of this theory is that Mackintosh (1975) considered the possibility that the salience factor might be specific for a certain reinforcement. This conjecture is based on previous findings (Mackintosh, 1973) that random presentation of the CS and the UCS prior to conditioning interferes with subsequent conditioning to these stimuli. This interfering effect, furthermore, is specific for the UCS used.

Starting from these suggestions, then, it could be argued that our subjects through earlier experience have been tuned to attend to potentially phobic stimuli in connection with aversive but not with nonaversive events, whereas for fear-irrelevant stimuli there is no previous experience prompting attention either with aversive or nonaversive stimuli. In general, therefore, this account could handle the data from all three experiments in the present series. However, it still has difficulties with some aspects of the results. Thus, the theory deals obviously with rate of learning, whereas the data to be explained here involve failure to extinguish. Similarly, whereas the term *attention* would seem to indicate cognitive control, the electrodermal responses to potentially phobic stimuli seem to be resistant to cognitive manipulations (Öhman, Erixon, & Löfberg, 1975; Hugdahl & Öhman, Note 1).

Another learning-theory account of the results could be couched in somewhat looser cognitive terms, mainly inspired by Grant (1972). Thus, it could be argued that snakes and spiders are easy to associate with the shock UCS, since they are easily integrated into a cognitive scheme or image where, for instance, the animals bite the subject in the hand. For a tone UCS such as the one used in the RT group in Experiment 3, such schemes or images are less readily formed, and similarly, for the fear-irrelevant stimuli such images are more farfetched. This account, then, presumes that cognition and imagery are important for conditioning, and while direct data here are lacking, it is clear that imagery is a potent factor in other types of learning (e.g., Paivio, 1969) and in emotional arousal (e.g., Lang, Melamed, & Hart, 1970). This theory, similar to the one of Mackintosh (1975), however, does not explain why the effect is evident only during extinction, and why cognitive manipulations have such limited effects.

Turning to the theories specifically developed to handle taste-aversion learning, it seems as if our results are more difficult to explain in terms of theories toward the general associative mechanism end than in terms of theories toward the more specific biological end of the continuum (outlined in the introduction). Thus, from the standpoint of Revusky (1971), it would be expected that stimulus relevance is one factor behind the superior conditioning to phobic stimuli. This factor would exert its effect by protecting the phobic–shock association from interfering events, whereas such interference would be more likely in the fear-irrelevant–shock association. However, unless the interfering events can be independently specified, this explanation remains entirely post hoc. The mechanism for protection of taste–illness associations from interference is hypothesized to reside in the different locus of the associative and interfering events. Thus, the former are interoceptive and the latter most likely exteroceptive. In our case, however, all events are exteroceptive, and therefore it is difficult to see why interference should affect one association but not the other. The only way for Revusky (1971) to account for our data would seem to be to assume that stimulus relevance per se facilitates associations, and then his theory clearly approaches the more biologically oriented alternatives. Similarly, Testa's (1974, 1975) proposition that general similarities between the CS and the UCS in terms of location and temporal intensity patterns would seem unable to explain our data, since all our stimuli were located "out there," and the temporal intensity patterns were identical for the phobic and the fear-irrelevant conditions.

For the more biologically oriented theoretical alternatives, it is clear, first, that belongingness is the very stuff that this type of theory is made of. Thus, belongingness is regarded as the rule rather than the exception (see e.g., Rozin & Kalat, 1971; Shettleworth, 1972). Similarly, the failure of phobic CSs to extinguish despite no difference from fear-irrelevant CSs during acquisition is a minor problem because, as argued by Rozin and Kalat (1971), extinction may be regarded as an independent adaptive feature of learning specifically tailored to ecological demands. Thus, where potentially fatal consequences are involved, as might be the case with at least some phobic situations (Marks, 1969; Seligman, 1971), the cost of omission is very high, and therefore, it would seem adaptive for fear to fail to extinguish in such situations (see Rozin & Kalat, 1971).

Several aspects of our data show a surprising similarity to data from taste-aversion studies. First, we have demonstrated CS–UCS belongingness (cf. Garcia & Koelling, 1966). Second, we have observed complete resistance to extinction after only one reinforcement (Öhman, Eriksson, & Olofsson, 1975; cf. e.g., Revusky & Bedarf, 1967). Third, we have repeatedly failed to observe extinction (cf. Mineka et al., 1972). Fourth, we have demonstrated that the responses conditioned to phobic stimuli are "noncognitive" in the sense that they fail to be affected by instructed extinction (Öhman, Erixon, & Löfberg, 1975; Hugdahl & Öhman, Note 1), which parallels the alleged insensitivity of conditioned taste aversion to informational characteristics (Kalat & Rozin, 1972). Finally, the fact that the effect is most obvious for the FAR, which as discussed above is related to orienting activity, implies that the meaning or the perception of the CS somehow is changed. Rozin and Kalat (1971) have suggested that a similar thing happens in taste-aversion learning; that is, that the taste itself becomes aversive rather than merely signaling a state of illness, and furthermore, that this might be a factor behind the alleged "irrationality" of taste aversion (Rozin & Kalat, 1971, p. 478).

Thus, in general our results support Seligman's (1970) preparedness notion in demonstrating similarities between two instances of alleged prepared learning, although they seem to imply that different preparedness continua are needed for acquisition and resistance to extinction (cf. Seligman & Hager, 1972, p. 5). In supporting the preparedness theory, the results also argue against the more extreme view of adaptational specialization of learning exposed by Rozin and Kalat (1971) and Shettleworth (1972).

However, it is important to point out that the conclusion that our data support Seligman's (1970) preparedness theory must be taken with considerable caution, since it is difficult to decide whether the similarities imply a truly homologous relationship, that is, similarity in mechanism, or merely an analogous one, that is, similarity on a functional behavioral level. Further caution is called for by several recent findings, which seem to indicate that in some respects taste-aversion learning is not as clearly distinguishable from unprepared classical conditioning as was formerly thought. First, Taukulis and Revusky (1975) have demonstrated that the Rescorla and Wagner (1972) theory can be used to account for inhibitory conditioning to taste using compound CSs of taste and odor for illness as the unconditioned response (see also Best, 1975). Second, allegedly "complex" phenomena of conditioning such as second-order conditioning (Bond & Harland, 1975) have been demonstrated in taste-aversion learning situations, which militates against the supposed "primitive" nature of this form of learning (e.g., Kalat & Rozin, 1972). Third, Krane and Wagner (1975) have reported successful conditioning with taste as the CS and shock as the UCS, although it remains unclear whether they have demonstrated true taste-aversion learning or merely that rats can utilize taste stimuli as signals for shock. Finally, there are data demonstrating long-delay learning (up to an hour) in a conventional, supposedly unprepared task such as a T maze provided that the association is protected from interference during the delay (Lett, 1975). It remains debatable, therefore, to what extent and in which role biological–genetic factors are needed to explain taste-aversion learning and, thus by implication, our results. At any rate, it seems to us a remote possibility that our data should have a bearing on, for example, the acquisition of language or imprinting—as is implied in the preparedness theory.

In conclusion, then, it is clear that our data show that the premise of equipotentiality is untenable in human conditioning, that the data are not easily accounted for by existing learning-theory alternatives, but that they are in accordance with the expectations from the preparedness theory (Seligman, 1970, 1971). It remains to be determined, however, to what extent biological factors are needed in the explanation, and whether they show similarities also to other instances of alleged prepared learning.

As a final point, it is noteworthy that the results from the present series of studies provide a basis for a more adequate understanding of the acquisition of phobias. Behavior therapists have faced a paradoxical dilemma with regard to these disorders. On the one hand, they have had efficient treatment methods that have been ostensibly based on conditioning principles, whereas on the other hand, these very same principles have been insufficient for a convincing account of the etiology of the disorder (e.g., Costello, 1970; Seligman, 1971). In several respects, the electrodermal responses conditioned to phobic stimuli resemble the characteristics of phobic fear. For example, electrodermal responses are very rapidly acquired, they fail to extinguish, and they are not easily modified by cognitive arguments. These results, then, would seem to be useful in developing an adequate laboratory analogue of phobias where, for instance, various determining factors and treatment methods could be studied utilizing full experimental control of the phenomenon under interest.

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ERRATUM TO HERSH AND CARAMAZZA

In the article "A Fuzzy Set Approach to Modifiers and Vagueness in Natural Language" by Harry M. Hersh and Alfonso Caramazza (*Journal of Experimental Psychology: General*, 1976, Vol. 105, No. 3, pp. 254–276), Equation 10 on page 261, which read

$$A + B = \int_X [f_A(x) \wedge f_B(x), x],$$

should read

$$A + B = \int_X [f_A(x) \vee f_B(x), x].$$

Also, Equation 11 on page 261, which read

$$f_A(x) \wedge f_B(x) = \max [f_A(x), f_B(x)],$$

should read

$$f_A(x) \vee f_B(x) = \max [f_A(x), f_B(x)].$$

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Visual Processing of Nonlinguistic Strings: Redundancy Effects and Reading Ability

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SUMMARY

Visual redundancy effects were investigated in two experiments using nonalphabetic material. Experiment 1 had adult subjects search for the presence or absence of a predetermined target symbol through single six-symbol linear arrays. Reaction times were significantly faster in both a distributional redundancy and a spatial redundancy condition than in a no-redundancy condition. Experiment 2 had good and poor sixth-grade readers serve in the no-redundancy and spatial redundancy conditions of Experiment 1. Poor readers were equivalent to good readers in the no-redundancy condition but were significantly slower in the spatial redundancy condition. Thus, spatial redundancy was shown to be operative in single symbol search time in a paradigm that avoided confounding by higher order linguistic variables. The present experiments, in conjunction with prior findings that good and poor readers differ in letter search time by their ability to use spatial redundancy, suggest that processes involved in the component skill of letter identification cannot be considered trivial to the reading process.

Mason (1975) provided evidence that spatial redundancy facilitates single-letter identification. Spatial redundancy was defined as a correlation between distinctive features of letters and the positions they most frequently occupy in printed English words. Good readers (Mason, 1975, Experiment 4) proved to be highly sensitive to the spatial redundancy of single letters in nonword anagrams, whereas poor readers were not. In two additional experiments (Mason, 1975, Experiments 2 and 3) good and poor readers were also differentiated at the level of single-letter identification by their ability to use spatial redundancy rather than by their ability to utilize distinctive feature information. The task used was a simplified version of the visual search task. The subjects were shown the letter they were to look for; they were then shown a single six-letter string. Their task was to look at the string and push a *yes* key if the letter was in the string and a *no* key if it was not present. The dependent variable was reaction time (RT) measured from onset of the display to response.

The question of whether higher order linguistic processes interact with lower order processes in single-letter identification has been the subject of much controversy (see, for example, Brewer, 1972). Perhaps the most unequivocal result Mason (1975, Experiments 2 and 3) obtained is that the particular higher order variable of dictionary inclusion or meaning is neither a necessary nor a sufficient condition for perceptual facilitation. Good readers were slightly faster searching for target letters through nonword anagrams of words than they were on the words themselves. The necessary condition was that the nonword anagrams be more spatially redundant than were the words. However, by manipulating single-letter positional frequencies as she did, Mason inevitably also varied bigram and trigram clusters of letters. Sequential redundancy is an ineluctable consequence of spatial redundancy: If *b* occurs most frequently in the first position of a six-letter string, and *a* occurs most frequently in the second position, there is a high probability of the sequence *ba*. The question is whether *b* and *a* are *independently* processed more rapidly because of their redundant positions in the linear array, or whether the sequence *ba* becomes a single perceptual unit. The notion that sequential redundancy facilitates perception by creating larger perceptual units than the single letter has had a long and a popular reign. However, when proper controls for decision facilitation are used, there are virtually no empirical data to support the notion of perceptual facilitation attributable to larger perceptual units (see Estes, 1975; Thompson & Massaro, 1973).

Given the available evidence, we must entertain the possibility that it is spatial redundancy that facilitates single-letter processing and that sequential redundancy is merely a result of spatial redundancy. Gough (1972) estimated a reading speed in excess of 300 words a minute based on serial processing of single letters. By altering his model to provide for parallel processing of single letters, and by having spatial redundancy influence iconic readout, it is possible to arrive at a model of highly proficient reading based on single-letter processing.

The goal is to isolate factors affecting single-letter identification. However, the proper study of letters may not be letters. A spatially low redundant arrangement of letters violates the good reader's knowledge of his language. Is the RT obtained for a good reader in a low spatial redundancy condition a measure of interference, or is it a measure of processing time based only on visual feature extraction? In addition, it is impossible with letters to manipulate spatial redundancy without affecting other variables such as pronounceability. Certainly the spatially redundant nonword strings were more pronounceable than the spatially nonredundant nonword strings used by Mason (1975) in Experiment 4. And certainly it is at least possible (albeit perhaps unreasonable) to argue that the spatially redundant nonword anagrams used in Experiments 2 and 3 were more pronounceable than the common words used to construct the anagrams. The issue of pronounceability is reducible to the issue of frequency. Given that the human vocal tract is physically capable of making certain sounds, a combination of sounds will be judged pronounceable or not pronounceable on the basis of frequency of occurrence. An unpronounceable phoneme combination in English may be highly pronounceable in Polish.

The extent to which visual information processing has been studied using only alphanumeric material is indeed surprising. It makes a separation of visual and linguistic factors exceedingly difficult. An alternative approach is to study redundancy factors affecting single-letter identification by using short linear arrays of symbols instead of letters. The purpose of the present two experiments was to extend the study of visual redundancy effects by using nonalphabetic material. Any redundancy effects obtained would then clearly be attributable to visual factors rather than to higher order linguistic variables. When employing a good- and poor-reader design, as is done in the second experiment, there is an additional advantage in using unfamiliar symbols instead of letters. Differential familiarity even with overlearned letters of the alphabet can be eliminated as a possible factor affecting the results.

An independent groups design is used in both Experiments 1 and 2. Experiment 1 uses adult subjects in two redundancy conditions and a pure no-redundancy control condition. The primary question of interest is simply whether redundancy effects are obtainable when short linear arrays of symbols are used. Experiment 2 investigates differences between good and poor readers in their sensitivity to spatial redundancy built into nonalphabetic stimuli.

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EXPERIMENT 1

Method

Subjects. The subjects were 36 undergraduates who participated in partial fulfillment of requirements for an introductory psychology course. There were 12 subjects randomly assigned to each of the three redundancy conditions: no redundancy, distributional (or frequency) redundancy, and spatial (or positional) redundancy. No attempt was made to control for reading ability, and we may assume a fairly broad range of reading ability in all three experimental conditions. Each subject was run individually, and returned for a second session within a time period ranging from 1 to 2 weeks. Insofar as circumstances permitted, subjects returned for their second session in the same sequence in which they had served in their first session. One subject was replaced due to an experimenter error.

Stimuli. The following 12 symbols were selected from the IBM typewriter symbol ball to form the basic set of elements used: δθξφλζψ}ΓΩ∂∫.

For the no-redundancy condition, 144 different six-symbol strings were generated in which each of the 12 symbols occurred 12 times in each of the six serial positions, and in which a particular symbol was preceded as many times as it was followed by any other one symbol. Thus, in the no-redundancy set of stimuli, it was impossible to differentiate the occurrence of one symbol from another symbol either on the basis of distributional redundancy, spatial redundancy, or sequential redundancy. Differentiation was possible only on the basis of visual features. In effect, the no-redundancy condition represents a maximum information condition and should not be confused with a random condition.

In the distributional redundancy condition, four of the symbols occurred 6 times in each of the six serial positions, four other symbols occurred 12 times in each of the six serial positions, and the remaining four symbols occurred 18 times in each of the six serial positions. Thus, the 12 symbols making up the 144 different six-symbol strings in the distributional redundancy set of stimuli could be differentiated on the basis of frequency, with one third of the symbols occurring three times as frequently in the displays as did another third of the symbols. Differentiation on the basis of spatial redundancy was not possible, however, since any one symbol occurred equally often in all six serial positions.

In the spatial redundancy condition, the 144 different six-symbol strings had each symbol occurring equally often (72 times each, as in the no-redundancy set of stimuli) but constraints were placed as to *where* the symbols could occur within the string. Two of the 12 symbols occurred only in the first serial position; another 2 symbols occurred only in the second serial position; 4 different symbols all occurred equally often in both the third and fourth position; 2 other symbols occurred only in the fifth position; and the remaining 2 symbols occurred only in the sixth position. Thus, in the spatial redundancy set of stimuli, symbols could be differentiated by positional constraints, but not on the basis of frequency, since all symbols occurred equally often in the displays. The pattern of positional constraints used roughly corresponds to the pattern of single-letter spatial redundancy found in printed English six-letter words (Mayzner & Tresselt, 1965), with maximum spatial redundancy present at both ends of the strings.

Each six-symbol string was typed on U film filmstrips and inserted into a slide mount. Individual target symbols appeared on a separate slide, each symbol centered on the slide. Three quasi-random blocks of target symbols and six-symbol display strings were arranged in slide trays for each condition. Each block of trials was equated for *yes* and *no* responses, serial positions, and symbols. In all three experimental conditions, each of the 12 symbols was tested 12 times: six times as a target present trial (*yes* response) and six times as a catch trial (*no* response); each serial position was tested 12 times, and the same sequence of serial positions tested and catch trials was used. Catch trials were identical for the three redundancy conditions both in sequence and in the particular symbol being tested. The sequence of target trials was identical for the no-redundancy and the distributional redundancy conditions, with these two conditions differing only in respect to the frequency of the symbols appearing in the displays. The sequence of target trials for the spatial redundancy condition differed from the one used for the no-redundancy and distributional redundancy conditions only in respect to the symbols tested in the different serial positions.

Procedure. A Kodak Carousel slide projector was used in conjunction with a Gerbrands tachistoscopic shutter (Model G1166), a Lafayette eight bank program timer (Model 52020), a Hunter digital clock (Model 1520), and two telegraph keys. One telegraph key was marked *yes* and the other *no*. Subjects used their dominant hand for *yes* responses and the other hand for *no* responses. The clock recorded elapsed time in milliseconds from onset of the stimulus display until the subject pressed one of the two keys. Target symbols and the six-symbol linear displays were back-projected with a Hudson Photographic Caritel. A Polaroid filter was attached to the projector lens to eliminate glare from the back-projection.

When the experimenter initiated a trial the target symbol slide appeared and remained on for 3 sec. Following a slide-change interval of approximately 1 sec, the six-symbol display slide appeared and remained on for 4 sec. After six practice trials with digits, the subjects were instructed that they would be asked to perform the same task, using unfamiliar symbols. Each symbol was then shown individually on the screen for 5 sec for purposes of familiarization. The subject then went through three sets (blocks) of 48 trials each, with a pause of about a minute between sets while the experimenter changed the slide tray. The order of presentation of sets was counterbalanced in a Latin square. Within each redundancy condition, four subjects had each of the three set orders.

The subjects were simply instructed to look for the target symbol when the six-symbol string appeared; speed was emphasized, but not at the expense of accuracy. An error was recorded when the wrong key was hit, and search time for that trial was eliminated. Subjects were informed when they made an error.

Results

Target trials. Six mean RTs (one for each serial position) were computed for each subject in each redundancy condition in each of the two sessions. Each mean was based on 12 RTs minus any error RTs for any one serial position. Serial position effects for the three redundancy conditions are shown in Figure 1. The similarity of the curves shown in Figure 1 is all the more remarkable since they represent independent groups of subjects. The analysis of variance showed a significant effect of conditions, $F(2, 33) = 4.436$, $p < .025$, that the no-redundancy group was slower than the two redundancy groups. Reaction times for all three groups were significantly faster in Session 2 than they were in Session 1, $F(1, 33) = 139$, $p < .001$. As to be expected from Figure 1, RTs varied significantly as a function of the serial position of the target symbols, $F(5, 165) = 39$, $p < .001$. The F ratios for all interactions were small (none greater than 1.5) and did not approach significance.

A trend analysis was performed on the combined data shown in Figure 1. The linear trend was significant, $F(1, 165) = 28.654$, $p < .001$, that RT was longest for the *first* serial position. The significant quadratic trend, $F(1, 165) = 155.287$, $p < .001$, confirms the impression from Figure 1 that RT was fastest for the middle positions of the strings. The cubic component was also significant, $F(1, 165) = 9.353$, $p < .001$.

Catch trials. A mean RT for catch trials was computed for each subject in each re-

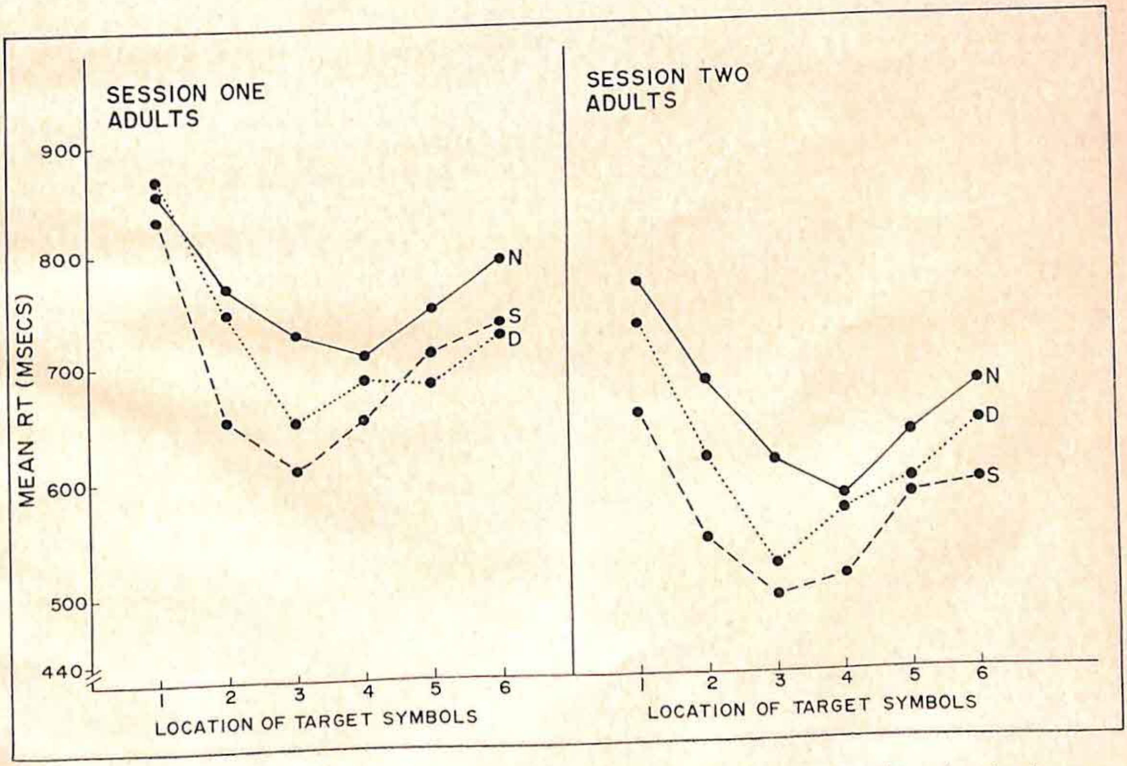

FIGURE 1. Mean reaction time on Experiment 1 target trials as a function of redundancy condition, serial position, and session. (N = no redundancy; D = distributional redundancy, S = spatial redundancy.)

TABLE 1

Mean Search Time (in Milliseconds) for Catch Trials in Experiment 1 (Adults) and Experiment 2 (Good and Poor Sixth-Grade Readers)

Redundancy condition	Session 1			Session 2		
	Adults	Good readers	Poor readers	Adults	Good readers	Poor readers
N	885	1,020	1,081	740	836	829
D	877	—	—	743	—	—
S	822	881	1,172	688	699	881

Note. N = no redundancy, D = distributional redundancy, S = spatial redundancy. There was no distributional redundancy condition for good and poor readers (Experiment 2).

dundancy condition for each session. Each mean was based on 72 catch trials, minus any error RTs. Mean group catch trial RTs are shown in the appropriate column in Table 1. The analysis of variance produced a significant effect only for sessions, $F(1, 33) = 131.87$, $p < .001$. The lack of a significant redundancy effect on catch trials supports the hypothesis (Mason, 1975) that redundancy plays its most important role in synthesis of graphic symbols. Synthesis is required only on target trials. Spatial redundancy was useful on catch trials for good readers at a low level of practice (Mason, 1975, Experiment 2). Given intensive practice (Experiment 3), good readers were able to process all three types of redundancy displays used equally rapidly.

Errors. The overall error rates were 8.5% for the no-redundancy group (12.5% misses, 4.5% false alarms); 7.3% for the distributional redundancy group (10.5% misses, 4.0% false alarms), and 7.8% for the spatial redundancy group (11.5% misses, 4.0% false alarms). Errors on target trials (misses) paralleled target trial RTs, with more errors occurring in the first position than in the middle positions of the strings.

Discussion

The main purpose of Experiment 1 was to determine if perceptual facilitation would be obtained by redundancy built into short linear arrays of nonalphabetic material. The answer was clearly yes; redundancy effects show up quite rapidly. The results are all the more impressive because they were obtained with an independent groups design; RTs are notoriously subject to much variation due to individual differences.

The serial position functions shown in Figure 1 are of particular interest. The subjects were seated directly in front of the screen and were given no processing instructions other than to "look." If they fixated directly in front of them (the easiest and most natural thing for them to do), the middle portions of the strings would have received the benefit of central foveal vision. Wolford (1975) presents a perturbation model for letter identification based on tachistoscopic report experiments in which retinal locus and array position were systematically varied. The main assumptions of the model are that features are extracted in parallel as a function of retinal location and that interference among confusable items increases as the items move into the periphery. The serial position functions shown in Figure 1, and derived from a very different paradigm, bear a remarkable resemblance to what the model would predict.

EXPERIMENT 2

Having determined that redundancy effects are rapidly obtained using nonlinguistic material, Experiment 2 investigates visual redundancy effects as a function of reader ability. The distributional redundancy condition was not considered to be sufficiently interesting or important to warrant inclusion in Experiment 2. There is evidence (Garner, 1962, chap. 8) that distributional redundancy is a minor effect, and there is also evidence (Katz, Mason, Woodward, & Wicklund, Note 1) that good and poor readers do not differ in their ability to use distributional redundancy. The critical redundancy conditions are the no-redundancy and the spatial redundancy conditions. The no-redundancy condition represents a condition in which symbol identification is possible *only* on the basis of visual feature extraction. If good and poor readers do not differ in their ability to learn and to use distinctive features of novel symbols, they should not differ in the no-redundancy condition. The spatial redundancy condition is critical because of the evidence (Mason, 1975) that good and poor readers can be differentiated at the level of the individual letter by their ability to use spatial redundancy. If good and poor readers differ in the spatial redundancy condition using nonlinguistic stimuli, then clearly the Mason (1975) results cannot be attributed to pronounceability, bigram and trigram clusters, or other higher order linguistic variables rather than to spatial redundancy of individual letters.

Method

Subjects. The subjects were 36 students drawn from a population of 238 sixth-grade students.[1] Eighteen pupils were selected as good readers and 18 pupils as poor readers. The selection procedure was based on Science Research Associates Achievement Series (SRA) scores taken at the end of the preceding school year. Each pupil's SRA reading comprehension score was used in conjunction with his SRA composite score. Good readers were pupils who scored 3 or more years above grade level on reading comprehension and whose reading comprehension scores were higher than their composite scores. Pupils with diagnosed learning disabilities were eliminated from the potential pool of poor readers. In addition, poor readers were pupils whose SRA reading comprehension scores were both below grade level and also lower than their composite scores. All the poor readers were either at or above grade level in at least one school subject on the SRA series other than reading comprehension. The rationale behind the selection procedure was to produce one group of pupils clearly talented in reading and another group of pupils whose reading comprehension was both below grade level and below their overall scholastic achievement.

Within each level of reading ability, two groups of nine pupils each were equated on reading comprehension scores and were randomly assigned to the no-redundancy and the spatial redundancy conditions. The mean reading comprehension score for the good readers was 9.7 with a standard deviation of 1.02 for those in the no-redundancy group and 9.7 with a standard deviation of 1.06 for those in the spatial redundancy group. The mean reading comprehension score for the poor readers was 3.7 with a standard deviation of .52 for those in the no-redundancy group and 3.8 with a standard deviation of .72 for those in the spatial redundancy group.

Five subjects were replaced: one good reader in the no-redundancy condition for failure to follow instructions, one poor reader in the no-redundancy condition due to equipment failure, two poor readers (one in each condition) who were unable to perform the task due to an inordinate difficulty in making the motor response with the correct hand, and one poor reader in the spatial redundancy condition whose absence from school for the second session coincided with the last school day before a week's vacation. The reading comprehension score data given above are based on the final 36 students who participated in the experiment.

Thirty-four of the subjects returned for their second session 1 week after their first session; two subjects (one in each reading group) had their second session 8 days after their first session.

Stimuli and procedure. The no-redundancy and the spatial redundancy sets of stimuli used for the adults in Experiment 1 were used for the sixth-graders. The procedure for Experiment 2 was identical to that used in Experiment 1.

Results

Target trials. Mean RTs for target trials at each serial position for each subject in each session were computed as in Experiment 1. Results are shown in Figure 2. The analysis of variance showed the differences between the four groups to be significant, $F(3, 32) = 4.742$, $p < .01$. Orthogonal comparisons showed the good readers in the spatial redundancy group to be significantly faster than the three other groups, $F(1, 32) = 13.953$, $p < .001$. Poor readers did not differ from good readers in the no-redundancy condition, $F < 1$. Poor readers in the spatial redundancy condition did not differ from good and poor readers in the no-redundancy condition, $F < 1$. All four groups were significantly faster on Session 2 than they were on Session 1, $F(1, 32) = 144.88$, $p < .001$. Reaction time varied as a function of serial position, $F(5, 160) = 23.03$, $p <$

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[1] The subjects were pupils at the Tolland Middle School, Tolland, Connecticut. The authors wish to thank Elaine Yatroussis, reading consultant, and Vene Harding, principal, for their active assistance and cooperation. A note of thanks is also due the nine sixth-grade teachers for their patience and forebearance.

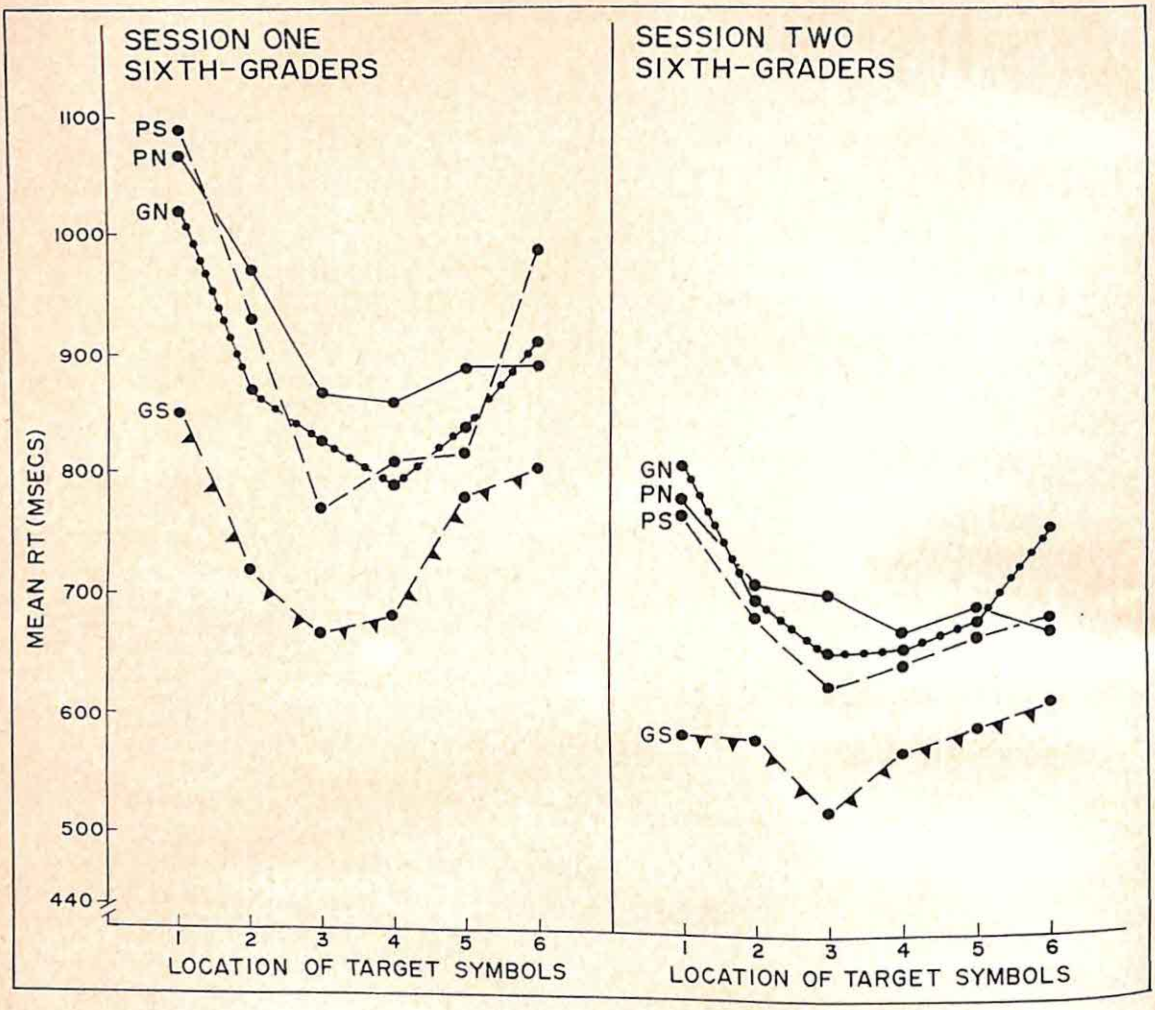

FIGURE 2. Mean reaction time on Experiment 2 target trials as a function of reader ability, redundancy condition, serial position, and session. (PN = poor readers, no redundancy; PS = poor readers, spatial redundancy; GN = good readers, no redundancy; GS = good readers, spatial redundancy.)

.001. The only significant interaction was that of Session × Serial Position, $F(5, 160) = 6.26$, $p < .001$. As can be seen from Figure 2, in contrast to the adult data, the children's serial position curves were generally flatter in Session 2 than in Session 1.

Because of the significant Session × Serial Position interaction, a separate analysis of variance and trend analysis was conducted within each level of reading ability for each session. For the two good-reader groups in Session 1, the main effect of redundancy was significant, $F(1, 16) = 5.317$, $p < .05$; spatial redundancy improved performance for good readers. The main effect of serial position was also significant, $F(5, 80) = 16.667$, $p < .001$. The trend analysis showed a significant linear component of serial position, $F(1, 80) = 5.755$, $p < .025$—RT was slowest on the first serial position. The only other significant trend was the quadratic component, $F(1, 80) = 73.157$, $p < .001$, that RTs were fastest when the target symbols occurred in the middle portions of the strings. The analysis of variance for the two poor-reader groups in Session 1 showed no effect of redundancy ($F < 1$) and only a significant main effect of serial position, $F(5, 80) = 13.154$, $p < .001$. The trend analysis showed a significant linear component, $F(1, 80) = 18.833$, $p < .001$, that RT was slowest for first serial position, and a significant quadratic component, $F(1, 80) = 50.917$, $p < .001$, that RTs were fastest when the target symbols occurred in the middle portions of the strings.

On Session 2, the analysis of variance for the two good-reader groups showed a more marked effect of redundancy, $F(1, 16) = 13.882$, $p < .005$; spatial redundancy benefited the good readers. The main effect of serial position was also significant, $F(5, 80) = 6.200$, $p < .001$. The trend analysis revealed that, in contrast to Session 1, only the quadratic component was significant, $F(1, 80) = 28.513$, $p < .001$. All other components were negligible with $F\text{s} < 1$. The analysis of variance for the two poor-reader groups in Session 2 again showed no effect of redundancy, $F < 1$, and only a main effect of serial position, $F(5, 80) = 4.857$, $p < .001$. The trend analysis for the poor readers again showed a significant linear component, $F(1, 80) = 12.780$, $p < .001$, and a significant quadratic component, $F(1, 80) = 10.109$, $p < .005$. All other components were negligible, with $F\text{s} < 1$.

Catch trials. Mean RTs for the 72 catch trials for each subject in each session were computed as in Experiment 1. Mean group catch trial RTs for the four groups are shown in the appropriate columns of Table 1. The analysis of variance showed significant differences between the four groups, $F(3, 32) = 5.206$, $p < .005$. Orthogonal comparisons showed that the good readers in the spatial redundancy group were significantly faster on catch trials than were the other three groups, $F(1, 32) = 12.822$, $p < .01$. Good and poor readers did not differ significantly in the no-redundancy groups, $F < 1$. Poor readers in the spatial redundancy group did not differ from the readers in the two no-redundancy groups, $F(1, 32) = 2.559$, *ns*. Thus, it would appear that spatial redundancy benefited the good readers on catch trials as well as on target trials. This is in contrast to the finding (Mason, 1975, Experiments 2 and 3) that spatial redundancy was effective on catch trials for good readers only at a low level of practice. The explanation probably lies in differential effects of practice with overlearned stimuli such as letters and with the novel stimuli used in the present experiment. The difference between the adults and the good readers in the use of spatial redundancy on catch trials may be due to developmental differences in the ability to make the most effective use of the distinctive features of the symbols in a given number of trials.

The main effect of sessions was significant, $F(1, 32) = 132.571$, $p < .001$. The Session × Reader Ability interaction was also significant, $F(1, 32) = 4.857$, $p < .05$. As can be seen from Table 1, overall differences due to reader ability were greater in Session 1 than they were in Session 2.

Errors. The overall error rates were 14.0% for the good readers in the no-redundancy group (17.0%, misses, 11.0% false alarms); 16.2% for the poor readers in the no-redundancy group (20.4% misses, 12.0% false alarms); 15.2% for the good readers in the spatial redundancy group (20.0% misses, 10.4% false alarms); and 16.8% for the poor readers in the spatial redundancy group (18.1% misses, 15.4% false alarms). In contrast to the adults, the sixth graders made enough miss errors to permit an analysis of variance. The only significant effect to emerge from the analysis was that of serial position, $F(5, 160) = 33.375$, $p < .001$. The pattern of errors by serial position closely paralleled the analysis of RTs by serial position: Most errors occurred in the first position; fewest errors occurred in the third and fourth serial positions.

Discussion

The results of Experiment 2 are straightforward: Spatial redundancy (a correlation between distinctive features and their spatial location within a string) facilitates single-symbol processing for good readers both on target trials and on catch trials. The efficacy of spatial redundancy on catch trials would seem to depend on how much processing is done of the symbols making up the display. This, in turn, would seem to depend on the ability to extract the most salient distinctive feature(s) of the target symbol. In Experiment 1 redundancy did not help the adults on catch trials. Spatial redundancy also did not help highly practiced good sixth-grade readers on catch trials when letters were used (Mason, 1975, Experiment 3). Good readers at a low level of practice, however, were significantly helped by spatial redundancy on catch trials when letters were used (Mason, 1975, Experiment 2).

In contrast to the good readers, poor readers showed no facilitation in single-symbol processing attributable to spatial redundancy. Yet poor readers were equivalent to good readers in the no-redundancy condition, when symbol identification was possible only on the basis of visual feature information.

The obtained results for the good readers obviously cannot be attributed to higher order linguistic variables. Thus, they provide strong evidence that similar results obtained with letters (Mason, 1975) were indeed due to spatial redundancy, the variable manipulated, rather than to higher order linguistic variables that change with spatial redundancy.

In a previous experiment (Mason, 1975, Experiment 3) highly practiced good readers showed only a significant quadratic effect of serial position, with RTs being *greatest* at the middle portions of the string. The longer RTs were attributed to lateral masking by adjacent letters (Townsend, Taylor, & Brown, 1971). On Session 2 in the present experiment, the good readers again show only a significant quadratic effect of serial position. However, RTs are now *shortest* in the middle portions of the strings. It is difficult to account for this discrepancy. The only procedural change in the two experiments was the addition of a Polaroid filter on the projection lens for the present experiments. Conceivably the results from the earlier experiment were due to illumination differences rather than to lateral masking. On the other hand, the earlier experiment employed the full set of 26 alphabet letters, whereas the present experiment used only 12 symbols. Data from detection experiments (Estes, 1972b) indicate that only confusable items interfere with one another because of competition for the same set of feature detectors. Thus, the opportunities for interference in the earlier experiments were greater than in the present experiments.

The obvious way to determine if illumination differences rather than lateral masking were responsible for RTs being elevated in the middle positions would be to replicate Experiment 3 of the earlier study using a Polaroid filter. The research effort, however, would not seem well spent. If the differences *were* due to illumination factors, then the obtained good- and poor-reader differences in both the earlier study and the present study could be generalized over different illumination conditions. There is as yet no satisfactory model of letter identification that can incorporate existing data on effects and interactions of retinal location, serial position, number of items, interference between similar items increasing as they move into the periphery, and lateral masking. In the absence of such a model, the finding that even with a filter RTs were still elevated in the middle positions would not be particularly meaningful.

A point that would seem more worthy of further research effort is the fact that highly practiced good sixth-grade readers in both the earlier study and in the present study differ from *both* adults and poor readers in the absence of a linear trend in their serial position functions. The serial position curve for the good readers in the spatial redundancy group shown in Session 2 of Figure 2 raises the interesting speculation that one role spatial redundancy may play in highly proficient reading is to minimize the effects of serial position and hence of retinal location in either single-letter or single-symbol identification. It should be remembered that the adults in Experiment 1 were not screened for reading ability. I would hazard the opinion that as a group the adults were not as outstandingly talented in reading as were the good sixth-grade readers in Experiment 2. Reading is a complex skill, and the differences between an average group of adults at a state university and a group of carefully selected good sixth-grade readers may be analogous to the differences between competency and virtuosity.

CONCLUSION

The reading of an alphabetic writing system is a highly complex skill that involves the integration and automatization of divers component skills. One such component skill is the ability to rapidly identify individual letters in a multiletter string. Is it an important skill as far as reading is concerned? How much reading research effort does it warrant? In the absence of hard data, the answers have largely depended upon theoretical formulations of the reading process and assumptions (more frequently implicit rather than explicit) about the basic perceptual unit involved in reading (i.e., the unit of text that is defined by a feature list and is stored in long-term memory). If reading is conceived of as a psycholinguistic guessing game (Goodman, 1970), then clearly the component skill of single-letter identification is relatively trivial. If higher order linguistic variables serve to create perceptual units larger than the individual letter, again the component skill of single-letter identification is not particularly important. If the individual letter is regarded as the basic perceptual unit involved in reading, then the component skill of single-letter identification is important, but only if it differentiates effective reading from ineffective reading.

Thompson and Massaro (1973) provided the first serious evidence that the individual letter is the basic perceptual unit involved in reading. Mason (1975) provided evidence that good and poor readers could be differentiated at the level of single-letter identification by their ability to use spatial redundancy rather than by their ability to use visual feature information. Spatial redundancy, as conceived of by Mason, is perceptual in nature and is not a higher order linguistic variable. Distinctive features represent fixed levels on physical dimensions. The position a letter occupies in a string of letters perceptually chunked by an initial and terminal blank space is also a physical dimension of the stimulus. Spatial redundancy refers to a positive correlation between these two physical dimensions. The second experiment reported in this paper built spatial redundancy into nonalphabetic materials. Again, good and poor readers were differentiated by their ability to use spatial redundancy rather than by their ability to use visual feature information. Since nonlinguistic materials were used, the results are clearly attributable to spatial redundancy rather than to a number of possible higher order linguistic variables that change with spatial redundancy when letters are used.

The importance of the present studies, in conjunction with the Mason (1975) studies, is that they make it clear that the particular component skill of single-letter identification *cannot* be considered trivial to the reading process. This is *not* to say that it is the only important component skill, the most important component skill, etc. Such a position would be absurd. The intent, rather, is to say that the component processes (themselves complex) involved in single-letter identification warrant serious reading research effort. A comprehensive model of single-letter identification may well have to include main effects and interactions of spatial redundancy along with other variables such as retinal location, position within the string, number of adjacent letters, shared distinctive features, and distance between shared distinctive features (see Estes, 1972b; Wolford, 1975). The difficult problem of a mechanism for handling "context" effects in single-letter identification could turn out to be a nonproblem, should it develop from future research that spatial redundancy rather than "context" is the relevant redundancy variable.

To conclude, the role of perception in reading has perhaps been underestimated because it is primarily the perception of form (what is it?) that has been investigated. Evidence from the no-redundancy groups in Experiment 2 of the present study support earlier indications that good and poor readers are comparable in their ability to use distinctive feature information (Katz & Wicklund, 1972; Mason, 1975). However, the perception of spatial location (where is it?) is quite distinct from the perception of form. We have behavioral data that item information and order information have separate representations in memory (Crossman, 1960; Estes, 1972a; Bjork & Healy, Note 2). We also have physiological evidence that form and space perception are mediated by different brain regions (Schneider, 1969; Trevarthen, 1968). The role in reading of the perception of the spatial location of letters has received very little research effort. It could well be that this has been a serious oversight. Spatial redundancy is based on the perception of where a letter is located in a linear array. There is some support for the notion of a spatial order perception deficit in poor readers. Mason, Katz, and Wicklund (1975) found that immediate spatial order memory (where was it?) for letters within strings was significantly better in good readers than it was in poor readers. They used a task which required retrieval only of order information; subjects were given letter tiles for all the letters that had been present in a just viewed string of letters. The task was to arrange the letter tiles in the order in which they had occurred in the string. A further study (Mason, Note 3) had good and poor readers use the appropriate tiles to simply reproduce the order of a stimulus string still in view (where does it belong?). Both letter strings and nonsense form strings were used. Poor readers were significantly slower than good readers in performing both types of tasks and also made more errors.

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Some Considerations of Two Alleged Kinds of Selective Attention

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SUMMARY

The present article deals with selective attention phenomena and elaborates on a stimulus material classification, "stimulus set" versus "response set," proposed by Broadbent (1970, 1971). Stimulus set is defined by some distinct and conspicuous physical properties that are inherent in the stimulus. Response set is characterized by the meaning it conveys, and thus its properties are determined by cognitive processing on the part of the organism. Broadbent's framework is related to Neisser's (1967) distinction between two perceptual–cognitive processes, namely, preattentive control and focal attention.

Three experiments are reported. A *before–after* paradigm was employed in Experiment 1, together with a spatial arrangement manipulation of relevant versus irrelevant stimuli (being grouped or mixed). The results indicated that *before–after* instructions had a stronger effect under stimulus set than under response set conditions. Spatial arrangement, on the other hand, affected performance under response set but not under stimulus set conditions. These results were interpreted as supporting the idea that stimulus set material, which is handled by preattentive mechanisms, may be processed in parallel, while response set material requires focal attention that is probably serial in nature. Experiment 2 used a search task with different levels of noise elements. Although subjects were not able to avoid completely the processing of noise elements, they had much more control under stimulus set than under response set conditions. Experiment 3 dealt with memory functions and suggests differential levels of perceptual processing depending on the nature of the stimulus material. This extends the memory framework suggested by Craik and Lockhart (1972). The results of these experiments, together with evidence from other behavioral and physiological studies, lend strong support to the proposed theory.

At the theoretical level, it is suggested that the distinction between stimulus and response set, and the corresponding one between preattentive mechanisms and focal attention, are on a continuum rather than being an all-or-none classification. Thus, it permits greater cognitive flexibility on the part of the organism, which is reflected through the assumption that both preattentive mechanisms and focal attention may operate simultaneously and differ only in the salience of their functioning. From a methodological point of view, the distinction between stimulus material and organismic processes is emphasized. It is argued that researchers have not given sufficient attention to the properties of the stimulus materials that they have used, and as a consequence have reached unwarranted conclusions, as exemplified by a few studies that are briefly discussed.

Broadbent (1970, 1971) recently suggested an important distinction between two kinds of selective attention. He argued that these two types of selection processes operate in different ways, and the failure to distinguish between them has resulted in some confusion in the literature.

According to Broadbent, in most experiments on selective attention the subject is provided (explicitly or implicitly) with a vocabulary of allowable responses and is presented with a variety of stimuli, only some of which are to be responded to. There are, however, two basic ways by which the distinction between relevant and irrelevant stimuli can be made. In one case, the vocabulary of responses determines the selection such that there exists a perfect isomorphism between the stimuli to be selected and the allowable responses: Irrelevant stimuli do not have any corresponding permissible responses. As an example, consider an experimental situation in which a subject is exposed to a mixed array of letters and digits and is instructed to report only the digits. Irrespective of whether or not his response is correct, he will almost never respond with a letter, since this is not included in the permissible set of responses. In the second type of selection, the irrelevant stimuli are perfectly appropriate for response, and the distinction between relevant and irrelevant stimuli is made on the basis of some other features that are not related to the class of appropriate responses. An example of such an experimental situation could be a case in which a subject is presented with a mixed array of red and black digits (or letters), and the task is to report either the red or black digits (letters). Here, the distinction is made on the basis of color, and there are no limitations on the set of appropriate responses. Following Broadbent's terminology, the first type of selection will be termed "response set" and the latter "stimulus set." It should be mentioned that while different situations of response set are very similar in the sense that in each case some limitation is made on the set of allowable responses, the nature of limitation will differ from case to case and probably will be involved in some different cognitive processes. For instance, differentiating between vowels and consonants probably requires a different operation from that of distinguishing between letters and digits, although both would be labeled as response set tasks. In the case of stimulus set, different distinctions can be made based on different features of the environment. For instance, the task could be responding differentially to lowercase or uppercase letters (in which the distinctive features would be size and shape); another case could be a distinction between stimuli that consist of circular versus straight-line elements (a shape distinction). In all these cases, the criterion that distinguishes between stimuli is a physical feature that is inherent in the stimulus itself. In the case of response set, on the other hand, the distinction is based more on the meaning that is attached to the stimulus, and the source of the distinction is within the organism (rather than in the environment). Crudely, the distinction (or selection) in the case of stimulus set is probably made at a relatively early perceptual stage of sensory registration; while in the case of response set, the selection is made at a relatively later stage, after the stimulus has been processed at least to some extent. Thus, Broadbent (1970, 1971) maintained that response set operates at a higher level of analysis than stimulus set. Similarly, Knight and Parkinson (1975)

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suggested that stimulus set is input selective, while response set is output selective.

Although the distinction between stimulus and response set is well defined in a formal sense, it is difficult to apply it in classifying real life situations. In most circumstances the distinction is not clear cut, since both stimulus and response set factors will be present. For instance, in the case of distinguishing between letters and digits, used as an example of a response set, one can argue that some very subtle shape differences may exist that might facilitate the distinction, thus providing the basis for stimulus set cues. Therefore, for practical purposes, the distinction between these two types of selection might more appropriately be made on a continuous relative scale than in a dichotomous, all-or-none fashion. Notice also that the "strength" or "efficiency" of stimulus set distinctions might vary. For example, a distinction between black and red is probably much more conspicuous and easier to handle than a distinction between black and gray, although both are considered to be stimulus set situations. The more conspicuous the physical properties of the environment, the more it will tend to be a stimulus set situation. Studies by Williams (1966), Von Wright (1968, 1970), and Snyder (1972) have explored in detail the relative efficiency of different stimulus set situations. The results of these studies suggest that location is the most efficient stimulus set cue, followed by color, size, and shape (in that order).

Broadbent (1970, 1971) argued that the above two selection types represent two different functions that to a great extent are independent, and he tried to explicate the differences between the two by using the framework of signal detection theory (TSD) (Coombs, Dawes, & Tversky, 1970; Green & Swets, 1966; Swets, 1964). Broadbent's assertion can be stated as follows: Stimulus set should affect d' (perceptual sensitivity), and response set should affect β (the response criterion). Before elaborating on this point, a brief discussion of the reasons that led Broadbent to this point of view is in order.

Broadbent's (1957, 1958) filter theory can be justified to serve as a natural starting point for any discussion of modern theories of attention (Kahneman, 1973). Without going into a detailed description of this model and its implications, several aspects of the model have been proved to be wrong. For one thing, contrary to filter theory, it has been shown that parallel processing of simultaneous stimuli does occur in certain tasks of divided attention (Treisman, 1970; Treisman & Fearnley, 1971). Furthermore, there is sufficient evidence demonstrating that the content of irrelevant stimuli is identified, at least dimly and at least some of the time, even when the subject attempts to ignore it (see for example, Eriksen & Eriksen, 1974; Eriksen & Hoffman, 1972, 1973). Finally, the idea of a slow-moving filter that selects one stimulus at a time is not proven viable (Kahneman, 1973).

In an attempt to accommodate the evidence against filter theory, Treisman (1960, 1964, 1969) proposed a modification that was subsequently accepted by Broadbent (Broadbent, 1970; Broadbent & Gregory, 1963). According to Treisman, the filtering was not necessarily an all-or-none process, which implied that rejected messages are merely attenuated, not eradicated. Briefly, Treisman postulated hypothetical "dictionary units" in memory, which are activated by sensory messages. Each unit has a threshold that must be exceeded for perception to occur. Such a model enables one, for example, to explain Moray's (1959) discovery that subjects often respond to their name spoken in one ear while they shadow a message in the other ear, simply on the assumption that thresholds for highly significant stimuli, such as one's name, are permanently lowered. An additional assumption is that thresholds for words that are highly probable within a certain context are temporarily lowered, thus explaining Treisman's finding that shadowing subjects occasionally follow the contents of a message that is suddenly switched from one ear to the other. Using TSD terminology, these assumptions refer to changes (permanent or temporary) in the decision criterion (β). On the other hand, differential treatments of attended and unattended stimuli (or channels) could be explained by differential sensitivity levels (d').

Accepting Treisman's (1960, 1964) attenuation model, Broadbent (1970) argued that "Response set should give increased correct reactions associated with increased false reactions to the same type, because an advantage conferred on some items in the dictionary by internal factors should apply regardless of the presence or absence of appropriate stimuli, while stimulus set should give increased correct reactions associated with decreased false alarms on the same channel, as stimulus set increases the effect of the objective situation" (p. 54). Putting it differently, response set should affect the response criterion by changing the relative importance of noise to signal. Stimulus set should be reflected in changes of d', that is, in sensitivity. Some indirect support for these assertions was provided by Broadbent (Broadbent, 1967; Broadbent & Gregory, 1963). To summarize then, TSD explicitly assumes a distinction between (a) the observer as a sensor, that is, the performance of sensory registration functions and (b) the observer as a decision maker, which is manifested in his values, expectations, and other cognitive processes and their effects on his responses (Coombs et al., 1970). These two dimensions are confounded in performance, and TSD supposedly makes it possible to distinguish precisely between the two aspects. In a similar manner, response and stimulus set characteristics are confounded in most real life situations. Thus, Broadbent (1970) suggested the similarity and ties between stimulus set and the first aspect of TSD mentioned above on one hand, and between response set and the second aspect on the other hand. The use of TSD is proposed in order to achieve a precise distinction between stimulus and response set.

The stimulus–response set dichotomy is not novel; it appears in the psychological literature under several terminologies. For example, the historical controversy between the two leading schools of psychology at the beginning of the century, the Structuralists and Functionalists, can be illuminated in terms of stimulus and response set. Attention, according to the Functionalists, implied an active stage on behalf of the organism, directing attention to this or that stimulus, and by so doing, determining the nature of its experience (James, 1890). The selection process, according to this view, involves increased mental processing of the stimuli and is dependent on some cognitive functions, thus requiring a more detailed analysis, which characterizes response set circumstances. The Structuralists, on the other hand, emphasized more selection processes that were of stimulus set nature. The essence of attention, according to the Structuralists, lay in what they called "sensory clearness," and they were mainly concerned with physical features of the stimulus that (using their terminology) were "standing out" (Titchener, 1908).

The concept of *attention*, following the Functionalists and Structuralists period, was practically banished from the psychological literature until the fifties when interest in the concept was renewed following Broadbent's (1957) article in which "filter theory" was first proposed. Again, one can trace the stimulus–response categorization that implicitly appears in several writings. For instance, following the terminology of Engel (1971) and Woodworth and Schlosberg (1965), one would include under stimulus set what they have termed "object factors" or "external determiners," while the nature of response set would be strongly connected to what they called "subject factors" or "internal determiners."

A very clear distinction between stimulus and response set can be obtained from Hebb's (1949) theoretical framework. Stimulus set attention in this context will operate on what Hebb termed the "primitive unity," which is a sensorily determined unity and is defined as "that unity and segregation from the background which seems to be a direct product of the pattern of sensory excitation and the inherited characteristics of the nervous system on which it acts" (p. 19). According to Hebb, the primitive unity is perceived independent of experience. Indeed, it is conceivable that the mechanisms underlying the operations of stimulus set nature are to a great extent innate. Some evidence for such an assertion can be found in Senden's (1932) monograph, which describes some blind patients who after the first occurrence of vision following operation for cataract always responded to certain objects as wholes and could on occasions detect differences between objects. The work of Hubel and Wiesel (1965, 1968) on different kinds of detectors also lends support to the possibility that mechanisms which operate under the conditions of what is termed in the current text as stimulus set are innate.

In short, attention under stimulus set conditions is manifested by the organism's capability to construct the global perceptual structure (like figure–background) of the environment at an early stage of sensory registration and to select certain part(s) of it for further processing. It is suggested that at least part of the mechanisms that control this process are probably innate, implying that little or no learning is involved.[1]

Response set would be characterized in Hebb's (1949) system by what he called the "nonsensory unity" and by "the identity of a perceived figure," both of which are affected by experience. In this case, some nonsensory factors would affect the processing and final selection. In other words, the selection criterion in this case is not based on external physical properties of the stimuli, but rather on the basis of some internal meaning that is conveyed by the stimulus or on some cognitive transformations through which it may go. Hebb explicitly mentioned that the primitive unity and the nonsensory unity are rather two extremes, and in most circumstances both sensory and nonsensory factors will affect the final construction of a percept. This is analogous to the remark made earlier with respect to stimulus and response set.

The closest theoretical framework to the present classification, and one which will be emphasized throughout this article, is the one which was proposed by Neisser (1967). According to Neisser's theory, perception is an active process of analysis by synthesis. Hence, a spoken message, for instance, is understood by covertly reproducing it, and visual percepts are produced by a similar activity of synthesis. However, as Neisser (1967) has noticed, "If we allow several figures to appear at once, the number of possible input configurations is so very large that a wholly parallel mechanism, giving a different output for each of them, is inconceivable. To cope with this difficulty, even a mechanical recognition system must have some way to select portions of the incoming information for detailed analysis. This immediately implies the existence of two levels of analysis: The preattentive mechanisms, which form segregated objects and help to direct further processing, and the act of focal attention, which makes more sophisticated analyses of the chosen object" (p. 94). Unfortunately, the exact nature of preattentive mechanisms and focal attention has never been well defined. Loosely speaking, preattentive mechanisms operate at a preliminary stage that involves rapid and fairly inaccurate processing. These "silent" systems probably operate on easily discriminable physical features, and are responsible for grouping and localization as well as routinely monitoring for critical features of stimulation that might require redirection of focal attention. According to Neisser, it is plausible to conceive of these operations as not being represented in awareness (cf. Kahneman, 1973). The subsequent "focal attentive" stage is a slower mechanism, acting upon less discriminable features of the stimuli and involving additional detailed processing. Unlike preattentive mechanisms, the operations of this level are said to be conscious and of a cognitive nature. In short, focal attention implies a higher level of analysis than preattentive mechanisms.

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[1] This is not to say that further experience might not shape the operations of these mechanisms. Experience would also be important in more complicated situations in which some of these basic mechanisms are needed for simultaneous operations (Rabbitt, 1967).

Broadbent's (1970) classification based on type of *stimulus material* can now be explicitly related to Neisser's (1967) theory of *perceptual mechanisms*. Indeed, the two frameworks are closely tied and are complementary in that the latter focuses on covert processes, while the former sets the boundaries and conditions under which these processes operate. It was mentioned earlier that Neisser did not provide rigorous and precise descriptions concerning the circumstances under which preattentive mechanisms will operate. Briefly, in the present context, it is suggested that preattentive processes will act on stimulus set conditions. No preattentive mechanisms can be developed for a pure response set, a condition which requires focal attention. It has been emphasized that the judgment of whether a specific condition is of stimulus or response set nature should be made on a relative scale. Similarly, the preattentive–focal attention distinction is not an absolute classification. Preattentive mechanisms will have a primary role in situations that are mainly determined by stimulus set conditions, and focal attention will have the primary role under response set conditions. Preattentive and focal attention are supposedly controlled by independent mechanisms; however, in most circumstances both operations will be present and will differ only in the salience of their functions.

In his final conclusions, Neisser (1967) suggested several hypotheses—which he admitted were rather speculative—with regard to the mechanisms and operations underlying preattentive mechanisms and focal attention, which further illuminate the connection with stimulus and response set, respectively.

First, the quick and fairly inaccurate scanning performed by preattentive mechanisms, and the fact that they encompass a relatively large part of the visual field, suggest that they might be working in parallel. Focal attention that is directed to a limited region of the field requires much more "concentration of psychic energy" (Neisser, 1967, p. 88) and is involved in a higher level of processing, which suggests that it is a more demanding process and probably serial in nature. It is suggested that in a similar manner, stimulus set material can be handled by parallel processing; in a response set condition, on the other hand, detailed processing is required and the stimulus undergoes cognitive transformations, which suggests a serial operation.

Second, focal attention processes are mostly acquired through experience whereas preattentive mechanisms, which are said to be subliminal, are in a way autonomatic and do not necessarily require previous experience. Indeed, it has already been suggested that some of the mechanisms controlling a stimulus set situation might be innate.[2] Hence, some learning is usually required for handling response set but not stimulus set material.

Finally, I suggest that focal attention, which involves higher level processing, makes it possible to encode the stimulus whereas the crude process of preattentive mechanisms do not.[3] The fact that stimulus material is encoded during focal attention processes but not under preattentive mechanisms (or differently, response set material is encoded but not stimulus set material) suggests differential effects on memory. Experiments 1 and 3 in the present article support this hypothesis.

In summary, it is suggested that the two frameworks of stimulus–response set and preattentive–focal attention are closely related, and that the former distinction might serve as a useful conceptual (or descriptive) framework for the latter. There are several advantages in using the terminology proposed in this article. First, it provides a precise definition for the domains under which Neisser's two different systems may operate. In addition, the use of stimulus–response set terminology might have important methodological implications, suggesting the need to avoid the confounding of stimulus set and response set conditions. Finally, the present framework might illuminate certain theoretical and controversial issues that have been raised in the current literature. Both the methodological and theoretical implications will be elaborated on in the final discussion.

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[2] As with stimulus set, this is not to say that the use of preattentive mechanisms cannot be more highly developed through experience, especially for more complicated tasks.

[3] The term *encoding* has received several different interpretations in the literature. In the present text, *encoding* is referred to as a covert process of constructing the percept, as a result of which it is stored in short-term memory. Actually, though, in the present context any of the current usages of the term would suffice and would not affect my argument.

Experimental Evidence

Turning to some experimental evidence, two experiments (one in the auditory modality and the other visual) were reported by Broadbent (1970) to demonstrate the difference between the two selection types that he proposed. The pre–post instruction paradigm was employed, a technique that has been used since Külpe (1904). In the present context the following predictions can be made: In the case of stimulus set, preinstruction will enable the system to reject irrelevant stimuli after analyzing very little information. According to Broadbent, the selection of red from black characters involves a single binary choice. For postinstruction, however, the subject must try to analyze fully all the stimuli presented and then select the relevant ones subsequently when he gets the instruction. Put differently, preattentive mechanisms might be of help in the preinstruction but not in the postinstruction condition. Hence, stimulus set should give a relatively large pre–post difference. For response set, on the other hand, a good deal of analysis (even of irrelevant items) is required both in the pre- and postinstructions. According to the present interpretation, the advantage of preattentive mechanisms cannot be utilized—focal analysis is required under both pre- and postinstruction conditions. Hence, response set should result in a relatively small pre–post difference.

In the visual condition of Broadbent's (1970) experiment, the stimuli were six digits ordered in two rows of three, one row being colored red, the other one white, for the stimulus set condition. For response set, digits versus letters were used, all appearing in the same color. Half of the cases had the upper row of digits and the lower row of letters; in the other half, the order was reversed. In the auditory condition, stimulus set consisted of male and female voices, while response set was again digits versus letters. Subject's task was to report all the characters that were in the category he was instructed to report. A between-subjects analysis design was used in both experiments.

Broadbent's results confirmed his expectations. Using an accuracy measure (percentage of lists that were correct), the difference between pre- and postinstruction was, in both modalities, significantly larger for stimulus set than for response set. This is an outcome of a sign test on the number of subjects showing a difference between pre- and posttest, which resulted in a significant difference for the stimulus set but not for the response set group. When, however, the pre–post difference for each subject was compared for the stimulus and response groups, it was significantly greater for stimulus set in the auditory condition ($p = .02$) but not for the visual condition. The insignificant effect in the visual condition is explained by Broadbent as being the result of the fact that different kinds of errors behave differently: "For error of intrusions from the wrong channel, there is a significantly larger pre–post difference for the stimulus set group while for other types of error the differences are almost identical. Substantial numbers of intrusions occur only under post cuing, and then as expected they are largely confined to the stimulus groups. In the response groups, the intrusions take the form of responses containing only items from the wrong class rather than a mixture of classes, and may be regarded as a failure to obey the instructions for the particular list" (Broadbent, 1970, p. 58). It is not clear, however, why this argument could not be equally applied to the auditory case. It is true that to identify errors of intrusion in the stimulus set condition is more difficult, since any response is included in the allowable set of responses, but the above argument is not satisfactory in explaining why the results in the auditory condition were significant and the results for the visual condition were not.

Additional disturbing facts, which Broadbent was aware of, are the results of an earlier study by Brown (1960). Although Brown's design was somewhat different, there are many analogies between the two studies, and Conditions II and IV in his study correspond to Broadbent's response set and stimulus set conditions, respectively. Brown, too, used a pre–post instruction paradigm and employed a between-subjects design. One of the results obtained from Brown's study is that the difference in performance between pre- and postinstruction was the same for both stimulus set (red or black characters) and response set (letters or digits). Thus, the results obtained from the two studies are incongruent.

Although the source of the discrepancy between the results obtained by Brown and Broadbent is not clear, they both can be criticized on some methodological grounds. The major drawback of both experiments was the use of a between-subjects design and a small number of trials for each subject (as well as a small number of subjects). The choice of such a design results in a low-power test as Broadbent himself has admitted. In addition, the between-subjects design does not enable a comparison to be made of the same subject on all conditions, and hence variance due to individual differences will be larger compared to a within-subjects design. With respect to the specific task, the use of a small number of trials is not sufficient to obtain a stable rate of performance. Finally, in the visual part of the experiment, the response set condition had a confounding effect of stimulus set, since digits and letters were always in the upper or lower row, and hence the row location might have served as a stimulus set cue.

Because of these methodological problems and the conflicting results reported by Broadbent (1970) and Brown (1960), it would be desirable to obtain additional data to clarify the issue by using a modified experimental design. Hence, Experiment 1 was designed to determine whether indeed the difference between pre–post instructions would be larger in the case of stimulus set than in the case of response set. In addition, another variable, namely the spatial arrangement of target and noise characters, was manipulated in order to get further understanding as to the nature of the processes underlying each of the selection types. A recent article by Fryklund (1975) indicates that the arrangement of the cued items and the background material is of importance when a partial report paradigm is used. In the present experiment it is expected that the arrangement of the noise items should not affect the stimulus set condition because preattentive mechanisms, which supposedly operate in this case, are crude and superficial and enable focusing on the targets, thus avoiding processing of noise material. For response set, however, spatial arrangement should be of importance because of the detailed analysis required in this case.

EXPERIMENT 1

Method

Subjects. Four undergraduate students from the University of Illinois (one male) served as paid subjects. All had normal or corrected-to-normal vision.

Apparatus and stimuli. A scientific prototype Model GA three-field tachistoscope was employed with Sylvania FT45/CWX lamps. Luminances of the three fields were set at 4.5 ft.L. (1.31 mc), as measured with a spectra spot photometer. A small black fixation cross appeared constantly in Field 1 (the adaptation field), except when the instruction or the stimulus displays were presented in Fields 2 and 3, respectively.

The stimuli consisted of the capital letters A, F, K, and N, which appeared either black or red, and the digits 2, 4, 5, and 8, which appeared in black (all taken from Lettraset Number 47-18-CLM); the black letters were mounted on white vinyl cards, and the digits and red letters were mounted on clear plastic cards. The display that was presented on each trial consisted of a combination of a white and a clear card. All stimuli were .175 in. (.344 cm) high, which subtended .18° of visual angle. Each display consisted of six characters positioned around an imaginary circle, 2° of visual angle in diameter, which was centered on the fixation cross. For the stimulus set condition, the display consisted of three red letters and three black letters; for the response set condition, the display consisted of three digits and three letters, all black. The set of characters to be reported appeared either adjacent to each other or separated (see Figure 1a and 1b). Sixty-four cards were prepared for the stimulus set condition (St) and sixty-four cards for the response condition (Re).

Thirty-two St cards consisted of three red letter combinations and 32 cards of three black letter combinations. The Re cards were divided into 32 letter and 32 digit combinations, all in black. Each block of 32 cards consisted of 16 cards in which the three characters were adjacent to each other; the other half consisted of characters that appeared separately. For each trial two cards were selected; the order of presentation was random and balanced such that each combination of three characters appeared equally likely.

The instructions as to which set of characters to report appeared in Field 2. These were capital letters (Para-Type Number 11505, height .4 in. [1.02 cm], which subtended .42° of visual angle) that appeared at the center of the imaginary circle. Since they were much larger than the stimulus characters, they were easy to perceive, and subjects had little difficulty handling the instruction after a short period of practice. In the stimulus set condition, the letter C was used to indicate report of colored letters (i.e., red), and the letter B was used for reporting the black letters. For response set, the letters D and L were used to indicate digits and letters, respectively.

Field 1 contained a black fixation cross that appeared in the middle of the field and subtended .2° of visual angle.

Procedure. There was a total of eight conditions: Stimulus set or response set, characters to be reported as adjacent or separated (this is the spatial arrangement variable), and instruction appearing before or after the stimulus display. Each of the four subjects was run for a total of 14 sessions, the first six of which served as practice sessions. A session was devoted entirely to the stimulus set condition or to the response set condition, and was divided into two blocks: In one block the instructions were given before, and in the other block instructions followed the display. Within each 48 trial blocks the instructions as well as the spatial arrangement of the stimuli were randomly distributed, under the restriction that each condition was equally likely; each experimental block was preceded by six practice trials. The order of sessions (i.e., whether it was a stimulus set or response set condition) and the order of blocks within a session (instructions given before or after) were counterbalanced among subjects.

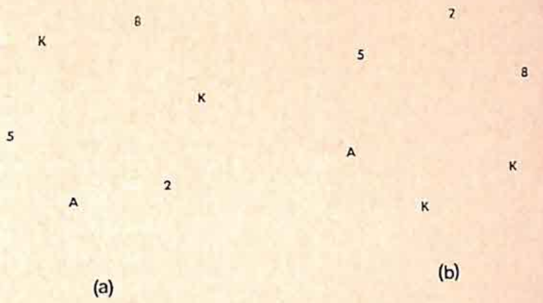

FIGURE 1. The set of characters to be reported (in this example, letter or digits) appeared either separated (a) or adjacent to each other (b).

Subjects were instructed to initiate a trial by pushing a button when they had the fixation cross in clear focus and were ready. In the *before* condition, the instruction letter appeared for 125 msec (such that the fixation cross was in the center of the letter) followed by 125 msec of the fixation field. The six-character stimulus display, which followed, appeared for a brief period that varied from subject to subject and was determined during the practice session (the criterion being 75% trials correct in the stimulus set condition where instruction appeared first). In the *after* condition, the order of presentation was reversed: The stimulus display appeared first followed by the fixation field (for 125 msec) and finally the instruction. The subject reported at the end of each trial the three target characters, and the experimenter confirmed a right answer or gave the correct response in the case of an error. Subjects were instructed to report three characters and to make their best guess in case they did not perceive all three characters. Each session lasted for approximately 45 min.

Results

The main results are summarized in Table 1, in which percent correct responses is shown for each of the two types of selection (stimulus versus response set) under *before* versus *after* instruction and

TABLE 1

MEAN PERCENT CORRECT RESPONSES FOR EACH CONDITION SUMMED ACROSS ALL FOUR SUBJECTS FOR EXPERIMENT 1

	Stimulus Set		Response Set	
Instruction	Adjacent	Separated	Adjacent	Separated
Before	84.9	78.1	82.3	66.9
After	47.4	35.9	57.0	42.7

the two different spatial arrangements of stimuli and noise. A three-way analysis of variance revealed that the main effects of instruction, $F(1, 3) = 154.3$, $p < .001$, and spatial arrangement, $F(1, 3) = 13.4$, $p < .035$, were significant. In addition, the interaction between type of selection and instruction was also significant, $F(1, 3) = 65$, $p < .004$. The significance of the instruction effect usually is considered as evidence of selectivity in the intake of information and, thus, confirms that some attentional processes have been employed. The significant interaction between selection type and type of instruction confirm Broadbent's (1970) assertion that the difference between pre- and postinstruction should be larger for stimulus set than for response set. Finally, the significant result obtained for the spatial arrangement is congruent with Fryklund's (1975) results, although she did not distinguish between stimulus and response set conditions. As will be shown, this effect is more salient under response set conditions.

Several studies in recent years (Dwyer, 1974; Hays, 1963; Humphreys & Fleishman, 1974; Vaughan & Corballis, 1969) have emphasized the importance of an additional measure that will indicate the proportion of variance accounted for by each factor. The most popular measure is usually ω^2, which was suggested by Hays (1963). Applying this measure to the data reveals that the instruction factor accounts for 68% of the variance, while the spatial arrangement and the interaction between selection type and the instruction factor (both of which turned out to be significant) account approximately for only 8% and 4% of the variance, respectively. The reason for this state of affairs is clear: The "obvious" *before–after* factor is so strong that it is masking the possible existence of other effects. In other words, the present situation can be interpreted in two ways. One possibility is that the above two factors are indeed of secondary importance and, hence, their low ω^2. There is, however, another possibility: It might be that the low ω^2 is simply an artifact that is created by the existence of another strong effect (in this case, the pre–post instruction).

It is possible to show that ω^2 is indeed a correlation coefficient, as was pointed out by Humphreys and Fleishman (1974). In order to find the real importance of a variable (or its strength of relationship with the dependent variable), the partial correlation of that variable with the dependent variable would be of interest. The use of a partial correlation will supposedly remove the masking effect produced by another variable that is interacting in the design. Humphreys and Fleishman (1974) suggested that ϵ^2 would serve as the appropriate measure for a partial correlation.[4] The formula for computing ϵ^2 is:

$$\epsilon^2 = \frac{SS_{\text{source}} - MS_e}{SS_{\text{source}} + SS_e + MS_e}.$$

Using this measure to indicate the "importance" of a factor, one obtains ϵ^2 equal to .71 for spatial arrangement and .93 for the interaction of instruction and selection type. The conclusion is that both effects are probably stronger than the ω^2 analysis indicates.

There is an additional way to break down the data. Since the experiment was conducted such that sessions were alternated between stimulus set and response set conditions, the data for each condition are independent. Hence, it was decided to analyze the data from each condition separately. Although the *before–after* effect was significant in both analyses, the variance accounted for by this effect differed in both conditions: For stimulus set, $F(1, 3) = 273.1$, $p = .0005$, $\omega^2 = .8452$, $\epsilon^2 = .9820$; for response set, $F(1, 3) = 67.5$, $p = .004$, $\omega^2 = .5152$, $\epsilon^2 = .9001$. Hence, there is additional support for the assertion that the difference between pre–post instruction is larger for stimulus set than for response set.

The separated analyses of the two conditions reveal another finding that is congruent with our expectations. As can be seen from the following data, the spatial location variable is significant for the case of response set, $F(1, 3) = 32.4$, $p = .011$, $\omega^2 = .1828$, $\epsilon^2 = .8626$, but not for the case of stimulus set, $F(1, 3) = 3.85$, $p = .1442$, $\omega^2 = .0326$, $\epsilon^2 = .3637$.

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[4] For a detailed discussion, see Keren & Lewis, Note 1. Also see Keppel's (1973) discussion on magnitude of treatment effects.

Discussion

The results of Experiment 1 confirmed Broadbent's (1970) findings and suggest that the two types of stimulus material are processed differently. There is also ample indirect evidence to support this finding, as will be reviewed later in this article.

Although the pre–post instructions affect stimulus and response sets differently, the paradigm does not reveal the differences in the nature of the processes underlying the two situations. The spatial arrangement variable was used to further investigate the different processes underlying the two selection types. If indeed stimulus set requires only a single binary choice between relevant and irrelevant stimuli, then spatial arrangement should have very little effect, if any. For stimulus set, the relevant stimuli should "stand out" irrespective of their spatial arrangement. For response set, however, the condition where noise and target letters are mixed should be more difficult than in the case in which they are grouped. The main reason is that the response set condition supposedly requires focal attention, and hence each character has to be analyzed in detail. Assuming that the subject is processing the six characters one by one (irrespective of the character on which he starts), he will always have to analyze at least five characters in the case where targets are mixed with noise letters; but for the case where targets are adjacent, he might sometimes start immediately with a target, in which case only three characters will be processed. In other words, under the assumption of serial processing, the average number of stimuli that needs to be processed is smaller under the adjacent condition compared with the mixed condition. Hence, differential reaction times under response set for the adjacent and separated conditions would be expected. The fact that the spatial arrangement effect was significant for response set but not for stimulus set supports these conclusions.

The results of Experiment 1 have further important implications. In a recent article by Shiffrin and Gardner (1972), two basic questions concerning the nature of visual information processing were raised:

1. Can the amount and rate of information processed from a briefly presented image at a given location in the visual field be varied by subjects voluntarily?
2. Is the information processed from a given location of a briefly presented display affected by the simultaneous processing of information from other locations in the display?

According to Shiffrin and Gardner, there is no definite answer to either of the above two questions. Studies by Estes and his coworkers (Estes & Taylor, 1964, 1966; Estes & Wessel, 1966) and by Rumelhart (1970) imply a *yes* answer to both questions. On the other hand, empirical and theoretical work by Eriksen and Spencer (1969), Eriksen and Hoffman (1972), and Gardner (1970, 1973) argues strongly that the answer to both is *no*. A recent article by Eriksen and Eriksen (1974) concluded that "S cannot prevent processing of noise letters occurring within about 1 degree of the target due to the nature of processing channel capacity and must inhibit his response until he is able to discriminate exactly which letter is in the target position" (p. 143).

Although the present point of view is close to that of Eriksen and Gardner, some modification is suggested. We would expect to find a difference with respect to the abovementioned two questions, depending upon the nature of the task that is used: With response set, I would definitely argue that the answer to both questions is *no*. With a typical stimulus set situation, the answer is not clear cut. The physical features might stand out and be sufficiently conspicuous to enable some control by the subject and allow him to avoid the processing of noise letters. Experiment 2 was designed to test this conclusion and further investigate the role of noise material under the two types of selection.

EXPERIMENT 2

The paradigm employed in the current experiment was similar to the one used by Eriksen and Eriksen (1974). Subjects detected one target among eight letters, and were instructed to press a lever to the right (or left) if the target letter was an A or an F and in the opposite direction for an E or a K. In the response set condition, half of the letters were consonants, the other half vowels. The subject was told before each trial to report either a vowel target (consonants being the irrelevant group) or a consonant target (vowels being the irrelevant group). For the stimulus set condition, black and red letters formed the relevant and irrelevant groups. Under both conditions the relevant and irrelevant groups consisted of four letters each; in addition to the four targets, another four "neutral" letters were used: U, O, N, and G. Under each condition the irrelevant group was manipulated in three ways: (a) The noise letters were drawn from the same set as the target and thus were response compatible. For instance, if in the response set condition, the subject was instructed to respond to vowels and the target was an E, the irrelevant group would include three Ks and an additional consonant from the neutral set. In the case of stimulus set, if the instruction was "black" and the target was a black A, the red letters would consist of 3 Fs (or 3 As) and an additional letter from the neutral set. (b) Noise letters for the irrelevant group were drawn from the opposite set of the target and were thus response incompatible. (c) The irrelevant group was neutral, and letters were drawn from the set U, O, N, and G. Thus each display was composed of the relevant set (one target and three neutral letters) and the irrelevant set (one of the above three conditions).

The effect of the three different noise conditions is of special interest. If response set requires more detailed and accurate processing, we might anticipate differential reaction times (as well as errors) under the three noise levels. Specifically, the incompatible noise condition should inhibit the correct response, which will yield slower reaction times and a higher rate of errors. The noise compatible condition should have a facilitating effect resulting in shorter reaction times and a low error rate. On the other hand, since selection under stimulus set conditions is said to be based on general and crude processing, the irrelevant noise letters will not benefit complete processing through to the point of encoding, and hence no differential performance under the three noise conditions should be expected in this case. The spatial arrangement of relevant and irrelevant letters was also manipulated in a way similar to that in Experiment 1.

Method

Subjects. Four undergraduate students from the University of Illinois (all males) served as paid subjects. All had normal or corrected-to-normal vision, and none had participated in Experiment 1.

Apparatus and stimuli. Stimuli and apparatus were essentially the same as in Experiment 1 except for the following: Each display consisted of eight characters that were positioned around an imaginary circle with 2° of visual angle in diameter centered on the fixation cross. Each display was formed by a combination of two plastic cards (one white and one transparent vinyl), each containing four letters. For stimulus set, red letters were mounted on one card, black letters on the other; for response set, four vowels and four consonants were mounted on each of the two cards, respectively. In addition, four instruction cards were prepared, two containing the words "black" and "red" (for the stimulus set condition), and the other two containing "vowel" and "consonants" (for response set). They were constructed from the same capital letters that were used in preparing the stimuli (Lettraset Number 47-18-CLN).

Procedure. Each subject participated in 10 sessions, the first two of which served for practice. A session was devoted entirely to either stimulus or response set conditions. Sessions were alternated each day and counterbalanced among subjects. Each session consisted of 6 warm-up trials followed by 96 experimental trials. Subjects were instructed to hold the hand switch to initiate the stimulus presentation in the left hand and to place the right hand on the hand rest with thumb and forefinger holding the response lever. They were told to be sure the fixation cross was in good focus before initiating a trial. Two of the subjects were instructed to press the lever to the right if the target letter was an A or an F and left for an E or a K. The other two subjects received the opposite instructions. Upon pressing the hand switch button, the fixation field went off and the instruction word ("vowels" or "consonants" for response set, "red" or "black" for stimulus set) appeared and remained for 750 msec. Upon termination of the instruction, the fixation field reappeared for 250 msec, followed by the stimulus field which stayed for 500 msec. Subjects were instructed to respond as quickly as possible while maintaining a high rate of accuracy. By the end of the trial, reaction time (starting from the onset of the stimulus field) was recorded, and the subject was informed about his exact reaction time. Any trials in which the subject pressed the lever in the wrong direction or both directions was not recorded and was rerun later in the session. A note was made of the error, and the subject was notified that his response was wrong. Each of the four target letters appeared equally often and they were randomly spread among the 96 trials. On every session, 32 trials were devoted to each of the three noise conditions and were randomized throughout the session.

Results

Since stimulus and response sets were tested during different sessions, there is sufficient ground to assume independence, and hence they were analyzed separately.

Reaction times. In Table 2 mean reaction times are shown for each of the three noise levels under adjacent and separate spatial arrangements and two different kinds of instruction (red versus black for stimulus set, vowels versus consonants for response set). A three-way repeated measures analysis of variance (ANOVA) was performed on each of the two conditions. For the response set condition, the only significant effect was type of noise, $F(2,\ 6) = 50.41$, $p < .0002$. A similar ANOVA performed on the stimulus set data also yielded a significant type of noise effect, $F(2,\ 6) = 13.84$, $p < .006$. The size of the F ratios suggest, however, that although the noise background had an effect on each of the two conditions, it had a much more important role under response set. The two measures of importance utilized earlier were employed to find out the variance accounted for by the noise effect under each condition, and the results were: for response set, $\omega^2 = .136$ and $\epsilon^2 = .926$; for stimulus set, $\omega^2 = .051$ and $\epsilon^2 = .769$. Finally, an ANOVA on the combined data from both the stimulus and response set conditions resulted in a highly significant noise effect, $F(2,\ 12) = 60.38$, $p < .00001$, and a significant interaction of Noise × Type of Stimulus Material (stimulus versus response set), $F(2,\ 12) = 11.66$, $p < .002$. Again, these results support the notion that the manipulation of the noise background had a much stronger effect under response compared with the stimulus set conditions.

Errors. The mean errors across subjects under the three conditions (type of noise, spatial arrangement, and kind of instructions) are summarized in Table 3.

Analyses identical to the ones performed on reaction time were used. The ANOVA on the stimulus set data indi-

TABLE 2

Mean Reaction Time for Stimulus and Response Set Conditions as a Function of Spatial Arrangement and Type of Noise for Experiment 2

Instruction	Adjacent			Separated		
	Neutral	Incompatible	Compatible	Neutral	Incompatible	Compatible
Stimulus set						
Red	676.7	717.1	705.9	695.2	754.7	730.5
Black	664.3	735.0	714.1	721.6	769.5	741.5
Response set						
Vowels	599.6	702.4	634.7	608.6	710.9	661.6
Consonants	667.0	812.8	702.1	687.8	848.2	727.5

TABLE 3

Mean Number of Errors and Percents for Stimulus and Response Set Conditions as a Function of Spatial Arrangement and Type of Noise for Experiment 2

Instruction	Adjacent			Separated		
	Neutral	Incompatible	Compatible	Neutral	Incompatible	Compatible
Stimulus set						
Red						
M error	2.50	2.25	2.00	1.50	2.75	1.75
% error	7.8%	7.0%	6.3%	4.7%	8.6%	5.5%
Black						
M error	3.00	2.75	3.25	3.00	2.50	2.25
% error	9.4%	8.6%	10.1%	9.4%	7.8%	7.0%
Response set						
Vowels						
M error	1.50	6.00	.75	1.75	4.25	2.00
% error	4.7%	18.8%	2.4%	5.5%	13.3%	6.3%
Consonants						
M error	2.75	4.00	2.00	1.00	9.25	2.00
% error	8.6%	12.5%	6.3%	3.1%	28.9%	6.3%

cated that none of the effects were significant. For response set, however, the noise background was highly significant, $F(2, 6) = 24.7$, $p < .002$. The rate of errors for the incompatible noise condition was significantly higher than for either the neutral or the compatible noise conditions, as revealed by a post hoc multiple comparisons analysis; the error rates under the latter two conditions were almost identical. The variance accounted for by the noise effect was high ($\omega^2 = .4374$, $\epsilon^2 = .8583$), thus suggesting that it played a critical role. The triple interaction between kind of noise, spatial arrangement, and kind of instruction was also significant, $p < .005$. Finally, an ANOVA on the combined data obtained from the stimulus and response set conditions yielded results identical to those obtained for reaction times; both the effect of noise, $F(2, 12) = 12.2$, $p < .002$, and the interaction of Noise × Type of Stimulus Material, $F(2, 12) = 10.4$, $p < .003$, were highly significant.

There was a difference in the mean percent of errors per subject (averaged across all other conditions) between the stimulus set (7.7%) and response set (9.7%) condition. Although this difference did not reach significance, it is in the expected direction.

Discussion

A fundamental question raised in Experiment 2 is whether perception of target items differs in any essential way from the perception of field items. Specifically, is it possible for subjects to attend selectively to the targets and thus "perceive" field items at a relatively rudimentary level, if at all? It was pointed out in the discussion of Experiment 1 that there is no unanimous agreement among researchers on this issue. In addition, the present point of view tends to agree with those researchers who concluded that a subject's ability to completely control his attention is unlikely. This article has earlier suggested a modification of this conclusion by differentiating between stimulus and response set conditions. The results of Experiment 2 strongly support such a modification. The significance of the noise effect (where reaction time served as the dependent variable) under both stimulus and response set conditions implies that the subject is not able to completely prevent the processing of field items in either condition. However, the relative importance of the noise effect (as measured by ω^2 and ϵ^2) suggests that more control on attention may be achieved by the subjects under stimulus set than under response set conditions. In other words, preattentive mechanisms are much more effective for selection than is focal attention, although even the former is not a perfect device. Additional support is obtained from the analysis of errors in which the noise effect was highly significant for response set but not for the stimulus set condition.

There are two disturbing results that were obtained in Experiment 2 and require additional explanation. The fact that reaction times under both stimulus and response conditions were fastest for the neutral noise background is somewhat surprising. Initially, it was expected that the best performance would be obtained in the compatible noise condition under the assumption that it will facilitate the response, since no response competition will be present; moreover, the correct response would be strengthened by the presence of additional characters from the same set (which are not present in the neutral condition). There must be, therefore, an additional factor that further facilitated the response in the neutral condition, resulting in performance superior to that under compatible noise background. It should be noted that the two sets of targets used in the experiment were highly similar: It is very difficult to distinguish between an E and an F and an A and a K that share common features.[5] It has been shown that the similarity of the target to the field has a marked influence on search rate, with search rate decreasing as similarity increases (Egeth, 1967; Neisser, 1963, 1964). Estes (1972) provides compelling evidence for a major role of intercharacter confusability. Because of the high similarity between the two target sets and the high confusability, additional time might be required for the response decision. However, the neutral set, which contained the letters O, U, G, and N, is easily distinguishable from the target letters. Thus, the neutral noise condition (under both stimulus and response set) had an additional stimulus set property, namely shape, which probably facilitated the process of directing attention to the target letter.

An additional unexpected result was the inability to replicate the spatial arrangement effect obtained in Experiment 1. There are two plausible explanations to account for this discrepancy. First, in Experiment 2 there were always three letters (out of the eight possible) that were identical. For instance, in the response set – compatible noise condition, if the target was an F, the irrelevant group would contain three As; similarly, for the incompatible noise, the irrelevant group would contain three Fs. It is plausible to assume that the three identical letters were conspicuous enough and "standing out"—again, in a way they formed a stimulus set property—so that the spatial arrangement effect was abolished. Such an explanation is congruent with Fryklund's (1975) conclusion that the spatial arrangement per se is not the critical factor; rather, the arrangement seems to control the degree of interference from the similar background material. In other words, the spatial arrangement effect might show under certain background conditions but not under others.

The failure to obtain the spatial arrangement effect could also be explained by the different nature of the tasks that were used in the two experiments. Experiment 1 employed a partial report paradigm, while in Experiment 2 a search task was used. Without reviewing in detail the different natures of the two tasks, it is plausible to assume that the demand for memory functions in a partial report paradigm is much larger than in the case of a detection task. Specifically, in Experiment 1 three characters had to be encoded and stored in short-term memory for later report, as opposed to a single letter that had to be detected in Experiment 2. One might speculate that the degree to which the spatial arrangement would affect performance depends on memory-demand characteristics. Although no experimental evidence to support this hypothesis is available, it should be noted that Fryklund (1975), who investigated the spatial arrangement effect extensively, used only a partial report paradigm.

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[5] This is according to Gibson's (1969) chart of distinctive features for a set of graphemes.

To summarize, then, it was demonstrated in Experiment 2 that noise material impairs performance to a much larger extent under response set than under stimulus set conditions. Supposedly, if stimulus set conditions exist, the preattentive mechanisms are able to prevent detailed processing except for the target; under response set conditions, preattentive mechanisms are not operating, and the subject cannot avoid the processing of irrelevant material.

It was suggested earlier that focal attention requires encoding of each stimulus (including irrelevant material), while preattentive mechanisms do not. Some support for this hypothesis is provided by Dick (1969). He used a partial report procedure in which groups of subjects were asked to report elements of a set of stimuli according to one of three attributes: spatial location, color (both of which serve as stimulus set), and class (letters and numbers for response set). His results are congruent with those obtained in Experiment 1, namely, different loss of information as a function of delay of instruction. He explained the results for the stimulus set conditions by a failure to transfer some of the information from the sensory register to short-term memory. In other words, irrelevant information is not encoded under stimulus set conditions. Experiment 3 was conducted in order to further substantiate this conclusion in an incidental learning paradigm.

EXPERIMENT 3

Method

Subjects. The subjects were 32 undergraduate students taking an introductory course in psychology at the University of Illinois. Participation partially fulfilled a course requirement.

General design. Subjects were assigned randomly into two equal groups, one serving in the response set and the other in the stimulus set condition. The response set group was again divided into two. Half of the subjects were instructed to search for letters and detect whether the target D or H was present; digits for this group were the irrelevant set. The other half was instructed to search for digits and detect whether the target 4 or 8 was present; letters for this group were the irrelevant set. The stimulus set group was also divided into two, half the subjects being instructed to search among black characters (i.e., red being the irrelevant set), the other half searching among red characters (i.e., black serving as the irrelevant set). Each subject, within these two stimulus set groups, was instructed to search for one, and only one, of the following targets: D, H, 4, or 8. This was done in order to equate the response and stimulus set conditions and make them comparable for the final analysis.

Apparatus and stimuli. The same apparatus employed in Experiments 1 and 2 was used. Each stimulus display contained eight characters and was prepared as follows: For the response set group that was to identify letters, the letters NEG (the relevant set) and the digits 3529 (the irrelevant set) were mounted on white vinyl plastic cards. The seven characters were ordered on an imaginary circle with eight positions (one position always left open for the target), which encompassed 2° of visual angle. Four cards were prepared, all containing the same seven characters and differing only in the order of the letters and the blank position left for the target (on two cards, letters and digits were mixed; on the other two, they were grouped). Another eight clear plastic cards, four of which contained the letter D and four of which contained the letter H, were prepared. Thus, any combination of a white and a clear card formed a stimulus, yielding eight possible different displays. Similarly, the backgrounds (i.e., the white vinyl card) for the group that was to identify digits contained the digits 359 (the relevant set) and the letters NEGP (the irrelevant set). The cards for the four stimulus set groups were prepared in a similar fashion except that half the characters were red, the other half black. In addition, response sheets (identical for all conditions) were prepared. The digits 1, 2, 3, 5, 6, 7, and 9 and the letters C, E, F, G, K, N, and P were printed in two columns; the subject had to circle the seven field characters he remembered appearing on the display.

Procedure. Subjects were told that they were going to participate in a visual perception experiment in which their task on each trial would be to detect whether a D or an H (a 4 or an 8) appeared among the eight characters. In the response set condition, they were explicitly told to search among the letters for a D or an H (or among the digits for a 4 or an 8) and ignore the irrelevant set, that is, the digits (letters). In the stimulus set condition, they were told to search for a D or an H (or for a 4 or an 8) among the black (red) characters, and ignore the irrelevant set, that is, the red (black) characters. Upon pressing the switch button, the fixation field disappeared and was replaced by a blank field that lasted for 150 msec and was followed by the stimulus display, which stayed on for 500 msec. After the termination of the trial, the subject had to report whether a D or an H (a 4 or an 8) was present. Subjects were forced to respond and were told to make their best guess in case they had not been able to detect the target. After the subject received the instructions and before he started the experimental trials, he was given three "dummy" trials in which the display contained eight 0s ordered in a circle in the same positions as the actual display. The purpose of this procedure was to introduce the subject to the pattern of the display and the exposure duration. The three dummy trials were followed by 20 experimental trials. Each of the eight possible displays appeared at least twice during the session, the order of presentation being random. The subject was given the response sheet immediately after the end of the last trial and the experimenter read the following instructions (which also appeared written on the response sheet): "Each display that you have seen contained a target and 7 other characters which were the same through all the presentations. Please indicate below (by circling the appropriate characters) which were the 7 additional characters according to the best of your memory." Subjects were required to circle seven characters and make their best guess in case they could not memorize all of them.

Results

The mean number of correct identifications on the recall test was 3.875 (*SD* = .873) for the stimulus set group and 4.688 (*SD* = 1.204) for the response set group (maximum possible correct responses being seven). As can be seen, the stimulus set group performance is almost on a chance level (3.5). A *t* test indicated that the difference between the two groups was significant ($p = .0368$), and the estimated variance accounted for by this effect was $\omega^2 = 10.5\%$. In order to reveal the source of difference between the two groups, an additional analysis was performed on the number of correct identifications from the irrelevant set. That is, for stimulus set, if the subject was instructed to search among black (red) characters, the number of correct identifications out of the four red (black) characters—which composed the irrelevant set—was taken; for response set, if the subject was instructed to search for digits (letters), the number of letters (digits) correctly identified was taken. The mean number of correct identifications out of the irrelevant set was 1.625 (*SD* = 1.025) for stimulus set and 2.25 (*SD* = .683) for response set.[6] The difference between the two groups was significant as indicated by a *t* test ($p < .05$), and the estimated variance accounted for by this effect was 8.9%. A similar analysis of the number of correct identifications from the relevant set indicated that the difference did not reach statistical significance ($p = .48$). The mean number of correct identifications (out of three) was 2.25 (*SD* = .775) and 2.438 (*SD* = .727) for stimulus set and response set, respectively.

Finally, an analysis was conducted on the number of errors committed during the 20 experimental trials previous to the recall test. Since the subjects did not have any practice trials, it is plausible to assume that at least the first few trials were serving as warm-up trials and probably inflated the variability. The mean number of errors for the response set group was 3.75 (*SD* = 2.517) and 2.500 (*SD* = 1.897) for the stimulus set group. Although the difference between the two groups did not reach statistical significance ($p = .0988$), it was definitely in the expected direction.

Discussion

Several articles (Bobrow & Bower, 1969; Rosenberg & Schiller, 1971; Schulman, 1971) support the general conclusion that memory performance is a positive function of the level of processing required by the orienting task (Craik & Lockhart, 1972). The fact that retention was indeed significantly better under response set condition supports the point of view taken in the present article, namely, that response set requires a more detailed and substantial processing compared with stimulus set. This conclusion is derived not only from the fact that retention under response set was superior to that under stimulus set conditions, but mainly from the fact that the major source of this difference lies in the retention of the irrelevant set. Those results support also the suggestion that irrelevant stimuli might be encoded under response set but not under stimulus set conditions. Additional evidence for the detailed processing under response set is derived from the rate of errors during the orienting task, which was larger for the response set condition. The lack of statistical significance can be explained by the small number of trials (20) and the fact that at least the first half of the trials served, in effect, as practice trials where guessing probably played a major role.

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[6] Notice that subjects in both the stimulus and response set conditions were informed that the display would contain both letters and digits but were not told that the number of letters and digits would be equal. Hence, no exact computations for chance is possible for the subanalyses, but only for the analyses of total errors. Since under both conditions most subjects immediately recognized 2 characters from the relevant set, they still had to indicate 5 characters out of 12. Although exact computations are not possible, it is clear that the number of correct identifications from the irrelevant set, if based on pure guessing, would be less than 2.

Keren (in press) has replicated the results reported here in a more elaborated experiment that employed the same paradigm and manipulated, in addition, the spatial arrangement variable. As expected, the spatial arrangement affected the response set but not the stimulus set conditions.

Additional Evidence for the Stimulus–Response Set Distinction

All three experiments, which were described above, converge on the same conclusion: Stimulus and response set situations should be treated differently. It should be emphasized again that in the present article, the distinction between stimulus–response set is treated as a classification that is based on the nature of the stimulus material. It is assumed here that the organism is handling these two types of material in different ways based on their different nature and the different requirements they impose on the organism; supposedly, the two kinds of stimulus material are controlled by different mechanisms and different processes. It was suggested that Neisser's (1967) approach is complementary to the stimulus–response set distinction, except that it is based on the organism's covert processes; thus, the two frameworks were combined here to form a theory of attentional processes. Crudely, response set material requires the higher level processing of focal attention; it might be termed a *process of cognitive selection*. Stimulus set material requires only a rough and general processing, which is performed by preattentive mechanisms. This kind of selection might be labeled *sensory perceptual selection*. In other words, stimulus set permits input selection; response set requires selection on the basis of information obtained from the perceptual mechanism.

Although the present article deals primarily—on both the theoretical and empirical level—with visual selective attention, there is ample evidence to extend our theoretical framework to the auditory modality as well. Neither Broadbent (1970, 1971) nor Neisser (1967) limited their approach to the visual modality. Broadbent (1970) conducted an analogous experiment to the visual one described in the introduction, which was intended to demonstrate the same findings on the auditory system. For the response set condition, again digits and letters were used. A male and a female voice were used for the stimulus set condition. The results were similar to those obtained in the visual modality: There was a large and significant difference in performance between pre- and postinstruction for the stimulus set condition but not for the response set condition.

A recent article by Knight and Parkinson (1975) provides additional support for the distinction between stimulus and response set in the auditory modality. In their experiments, subjects received dichotic lists comprised of word–digit pairs with instructions to recall either by spatial location (stimulus set) or by stimulus category (response set). Recall performance varied as a function of stimulus condition, response set resulting in more accurate recall than stimulus set, as would be expected. This finding that category reporting (response set) is superior to reporting by ear (stimulus set) is in accord with results of previous investigations (Katzin, Corballis, & Lockhart, 1972; Yntema & Trask, 1963). In addition to these findings, Knight and Parkinson studied the effect of a stimulus suffix (an additional acoustic element placed at the end of a stimulus list). They hypothesized that if indeed response set conditions require a higher level of analysis, then stimulus suffix effects should be less pronounced with response than with stimulus set. In the present context, response set material that is handled by focal attention is probably encoded and thus should be more resistant to external interference. Their results confirmed these expectations: Response set was shown to be less vulnerable to interference from a stimulus suffix compared with stimulus set. To put it in a different way, selection with stimulus set material is based on information at a "precategorical" level (Crowder & Morton, 1969).

On several occasions in the present article, it was argued that response set material—which supposedly is controlled by focal attention—requires a higher level of processing, that is, some kind of a cognitive activity. Most of the evidence for this assertion was implied indirectly from behavioral measures. There is, however, some physiological evidence that bears on this point and further supports this conclusion. Picton and Hillyard (1974) and Hillyard, Hink, Schwent, and Picton (1973) have reported that their studies entailed different evoked potentials for stimulus and response set. Hillyard et al. (1973) used an auditory task and concluded that "stimulus set attention appears to be reflected electrophysiologically in the enhanced N_1–P_2 response to all stimuli in the attended channel. Response-set attention is indexed by the generation of a P_3 complex to the target signal" (p. 178). Picton and Hillyard (1974) have defined stimulus set attention as involving the selection of a particular input channel to be examined in preference to others. Response set, on the other hand, involves the comparison of inputs in attended channels to "memory" or "template" units in order to recognize a selected target signal that requires special response. Though I do not completely agree with their definition, it is certainly congruent with the framework proposed in the present article.[7] With respect to the evoked potential components N_1–P_2 and P_3, researchers have not reached complete agreement as to the appropriate interpretation of these components and their nature. However, it is generally agreed that the N_1 and P_2 components depend more on stimulus parameters, that is, physical features, and represent activity of sensory registers (Davis & Zerlin, 1966; Picton & Hillyard, 1974; Ruhm, 1971), while the P_3 component represents higher processes of cognitive activity (Hillyard, 1969; Picton, Hillyard, & Galamtes, 1973; Squires, Hillyard, & Lindsay, 1973; Sutton, Braren, Zubin, & John, 1967).

Kahneman (1973) cited some physiological studies that support Neisser (1967), suggesting that "The distinction between preattentive and focal processes may be related to a distinction recently proposed between two functional visual systems: an orientation system concerned with the perception of space and with the detection of significant agents in the periphery of the field; and a central system concerned with fine discriminations" (p. 127). Physiological and comparative analyses of visual function in various animals provide much support for this distinction. Ingle (1967) studied motion detection by fish and revealed two separate processes that are hypothetically linked to different kinds of behavior. He concluded that "visual processes subserving 'orientation' of the fish to a moving object should be clearly distinguished from processes by which the fish 'evaluates' the identity or activity of the object" (Ingle, 1967, p. 50). Schneider (1967) reports a study on the golden hamster in which he was able to dissociate, by means of brain lesions, between visually guided orientation (spatial localization) and visual discrimination functions. Trevarthen (1968) conducted experiments with split-brain monkeys and suggested that vision of space and vision of object identity may be subserved by anatomically distinct brain mechanisms. He distinguished between two kinds of visual mechanisms: One class, which he labeled "ambient," supposedly determines space at large around the body, while the other, "focal," examines detail in small areas of space. Finally, studies on kittens by Held (1968) also support the hypothesis that localization and discrimination are controlled by different mechanisms. In summary, then, physiological research strongly supports and complements the behavioral research reported earlier.

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[7] The objection to this description refers mainly to response set and, in particular, to the assumption that under response set a comparison is made to a template or a memory unit. It also overlooks the cognitive operation that has to be performed on the stimulus.

Theoretical and Methodological Implications

Theoretical considerations. It was already mentioned that Broadbent had to revise his original filter model (Broadbent, 1958), since it was not able to account for several phenomena. He accepted Treisman's (1960, 1964) modification and complicated his earlier model by changing the selective filter from an all-or-none to an attenuation filter. In his last revision (Broadbent, 1971), the model was further elaborated. He postulated three different kinds of selection. One, which he termed "filtering," selects stimuli on the basis of some common physical features that they possess. A typical case of filtering is listening to hear which words are said by a particular person's voice. The second kind of selection, labeled "pigeon-holing," is based on the rule controlling the vocabulary from which the response will be chosen; for instance, listening to hear whether certain words are said, regardless of who says them. These two kinds of selection are closely connected to stimulus and response set. According to Broadbent (1971), "Filtering or stimulus set is the selection of certain items for analysis and response, on the basis of some common characteristic possessed by the desired stimuli. Pigeon-holing or response set is the selection of certain classes of response (category states) as having a high priority for occurrence even if the evidence in their favor is not specially high" (p. 177).[8]

The theoretical framework presented in this article differs in several respects from the one proposed by Broadbent (1971). A major source of these differences is that the present article makes an explicit distinction between the stimulus–response set classification (which is based on stimulus material) and the preattentive–focal attention classification (which is based on the nature of processes); Broadbent, on the other hand, never made an explicit distinction between stimulus–response set and filtering versus pigeon-holing (as exemplified in the above quotation), thus confusing physical states of the world (stimulus material) and organismic processes. Indeed, it is not at all clear to what extent filtering and pigeon-holing represent any processes, or whether they are just the final outcome of implicitly assumed operations. To put it differently, Broadbent's definitions are in terms of stimulus input and final response output, but he neglects the nature of the intervening processes between these two observable stages. The present article treats attention as some independent internal mechanisms (see Kahneman, 1973) and tries to investigate the nature of these covert processes.

There are several more specific differences between the two frameworks. According to Broadbent (1971), stimulus set is based on conspicuous physical features, and "The advantage of stimulus set lies in the ease with which irrelevant items can be discarded" (p. 178). I do agree that under stimulus set conditions, physical features play a most important role. However, stimulus set is not merely a quick scanning through the stimulus field, checking whether or not each item contains the specific feature. If this were the case, we would still expect to obtain a spatial arrangement effect in the stimulus set condition in Experiment 1. It was shown there, that for the response set condition, accuracy of the partial report decreased if relevant and irrelevant stimuli were separated rather than grouped. This result suggests a scanning process that is probably serial in nature. The fact that the spatial effect was not obtained under stimulus set conditions was interpreted as excluding a scanning process and suggesting that the nature of this operation is rather parallel. Following Neisser (1967) we see preattentive mechanisms as serving functions of grouping and segregating, thus, organizing the perceptual field (in terms of figure–background). Though preattentive processes certainly depend on physical features, it is not required that any single stimulus be "filtered" in Broadbent's terms.

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[8] The third type of selection is termed by Broadbent as "categorization," and is, in certain respects, just a special case of "pigeon-holing." Broadbent's discussion (especially when he is dealing with stimulus and response set) is limited only to "filtering" and pigeon-holing, and thus I will not elaborate further on the third type of selection.

The disagreement between the present article and Broadbent's (1971) approach is even larger with respect to response set. First of all, the main emphasis in Broadbent's definition of response set is that it limits the allowable vocabulary of response. This is true, though not necessarily applicable to all response set situations. More important, however, it does not refer to the processes that underlie this type of selection. Pigeon-holing is defined in terms of the final criterion (i.e., to which category does the stimulus belong) but fails to illuminate the process itself. The present article explicitly assumes that response set material requires some additional cognitive processes. While in the case of stimulus set, preattentive mechanisms are operating on the raw input (in philosophical terminology, on "sense data"), response set requires some cognitive transformation to be performed on the raw material. As a consequence of this cognitive operation, it has been shown that irrelevant material might impair performance under response set (see Experiment 2). Another consequence (demonstrated in Experiment 3) is that not all the irrelevant material is discarded under response set conditions. As a result of the necessarily detailed processing of the stimuli (including the irrelevant ones), they are encoded, and some irrelevant material is left in memory. The way Broadbent defines response set (and pigeon-holing) does not allow for predicting these results. Noise material should not affect the processing of relevant material, even if it is attenuated. Thus, pigeon-holing is perceived by Broadbent to some extent as a higher level filter, but no allowance is made for cognitive operations.

That response set is indeed perceived by Broadbent merely as a higher level filter can be seen from the following example. He states that stimulus set situations could vary in their case depending on the conspicuity of the physical features (a distinction made in the present framework). In a similar fashion he applies a similar distinction to response set: "Conversely, if the particular type face used for letters and digits is one which includes clear common features for one class then rejection of that class may be quick and easy. For example, in the following sequence the digits are, on an ordinary typewriter, all larger in size than the letters: wer2tui3oas4zx15vnms6" (Broadbent, 1971, p. 180). An alternative interpretation of this example is simply that stimulus set characteristics are sufficiently conspicuous to allow preattentive operations and, thus, facilitate the focal attention stage. It was emphasized that stimulus and response set, when applied to experimental situations would be considered on a relative scale, and that preattentive and focal processes are rather complementary. Broadbent's stimulus and response set (or alternatively, filtering and pigeon-holing) are operationally defined in terms of stimulus input and response output, ignoring intervening processes. As a consequence, the stimulus–response set dichotomy has the nature of all-or-none, and indeed Broadbent neglects to a great extent the aspect of relativity in the nature of the stimulus material and the complementary aspect of the underlying processes.[9]

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[9] Thus, Broadbent's attempt to equate TSD sensory and decision factors with stimulus and

Other theoretical frameworks for perceptual attention have also failed to account for the difference between the two kinds of stimulus material and the different internal mechanisms that operate on them. Deutch and Deutch (1963), for example, postulated that "a message will reach the same perceptual and discriminatory mechanisms whether attention is paid to it or not; and such information is then grouped or segregated by these mechanisms" (p. 83). How such grouping or segregation takes place is not explained by Deutch and Deutch. Thus, they ignore the existence of preattentive mechanisms. Though they assume parallel processing at primary stages (and in this respect their model is congruent with the assumption that preattentive mechanisms are operating in parallel), they do not elaborate on processes that are operating at this stage. One of the salient features of their system is "that it assumes that all sensory messages which impinge upon the organisms are perceptually analyzed at the highest level" (Deutch & Deutch, 1963, p. 85). Thus, they eliminate the possibility of selection based on a crude and nondetailed processing. They postulated a central structures system, which is somewhat similar to Triesman's (1960) dictionary units; each central structure has a weighting of importance that reflects momentary intentions or enduring dispositions, and among concurrently active structures only the one with the highest weighting of importance will be selected to control processing and eventually a response. How the selection was made is not clear. On one hand, the theory does not specify any kind of mechanisms that will be able to make selection on a crude and rather superficial level. On the other hand, the possibility of a detailed processing of all stimuli is also excluded, since they assert that "only the most important signals coming in will be acted on or remembered" (Deutch & Deutch, 1963, p. 84).

Kahneman (1973) also noticed the failure of the Deutch and Deutch model to account for the organism's ability to deal with stimulus set situations. He argued that Deutch and Deutch "assumed a system that can be preset in advance to favor the recognition of certain stimuli, such as animal names. However such a system cannot be preset in advance to favor words that will be heard on the right ear, since it has no knowledge of what those words will be. It can only favor a right ear word after all concurrent stimuli have activated the central structures to which they correspond. Thus, the process of selection by stimulus features appears to be more complex than selection by response class" (Kahneman, 1973, p. 124).

Kahneman (1973) has developed an approach that assumes that effort is mobilized in response to the changing demands of the tasks in which one engages. Each task requires a standard allocation of effort, which if not received will cause deterioration of performance. Without reviewing this approach in detail, it is sufficient to say that the theory does not enable us to make differential predictions with respect to stimulus and response set tasks. The present article argued that stimulus and response set material are treated by different mechanisms that to a certain extent are independent. The assumption that effort can be mobilized from one task to the other, specifically that effort allocated to preattentive mechanisms and to focal attention is interchangeable, is questionable.

The present framework also suggests a partial answer to two classic dilemmas of attention that are closely related. One is the question concerning the nature of processing: Is it serial or parallel? The other is the question of capacity, which was raised in the Discussion section of Experiment 2. Briefly, it is proposed that stimulus set material that is handled by preattentive mechanisms can be processed in parallel and has no capacity limits. Response set material requires a slow and detailed processing by focal attention that

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response set conditions seem rather weak, since both sensory and decision factors will operate under either condition.

is probably serial in nature and is bounded by capacity limits. However, most experimental situations are said to have both stimulus and response set characteristics, and the differences would only be relative. This implies that only in very few circumstances of extreme stimulus set or response set situations can one decide whether the nature of processing is parallel or serial, respectively. The fact that most experimental tasks involve both stimulus and response set properties suggests a possible reason for the confusion and the continuous controversy with respect to these two questions. The present framework permits the subject greater cognitive flexibility (Egeth, Atkinson, Gilmore, & Marcus, 1973), which is expressed through the assumptions that both preattentive and focal attention may operate at the same time and differ only in the salience of their functioning.

Methodological implications. Broadbent (1970) concluded his original article on the distinction between the two types of stimulus material with a warning: "Experiments on selective attention ought therefore to distinguish sharply between selection by stimulus set and selection by response set" (p. 58). Indeed, major implications of his article were of a methodological nature, warning of problems caused by confounding stimulus and response set situations. For instance, Broadbent reviewed the literature on pre- and postinstruction and found that the relative absence of difference reported by Lawrence and Laberge (1956), Lawrence and Coles (1954), and Pollack (1960) was always associated with the use of response set. On the other hand, positive results reported by Broadbent (1952) and Swets and Sewell (1961) were associated with the use of stimulus set.

A most important example is Sperling's (1960) influential monograph in which he introduced the partial report technique and demonstrated that more items are indeed available immediately after visual stimulus presentation than can be recalled. His stimuli were ordered in three lines (four letters in each), and the subject was instructed (by different frequency tones) as to what line to report. Thus the instruction guided the subject as to the location of the letters to be reported—by the present definition this is clearly a stimulus set task. When Sperling attempted to replicate his results in a response set condition, by instructing the subject to report letters from a mixture of letters and digits, he failed; even with instruction before stimulation, performance was so bad that postinstruction was regarded as impractical.

Posner and Mitchell (1967) provided a general experimental paradigm to observe depth of processing in simple classification tasks. In one of their experiments, subjects were briefly presented with letters on two-in-line displays. They were instructed to respond on each trial whether the letters were "same" or "different" and to press a lever accordingly. There were two levels of instruction: Under Level 1 (physical identity), subjects were instructed to classify letters "same" only if they were physically identical. Under Level 2 (name identity), they were instructed to respond "same" whenever the two characters had the same name (like A or a) and "different" if they did not. Posner and Mitchell have shown that reaction times (for both *same* and *different*) were significantly slower for name identity compared with physical identity. They concluded that each of the two instructions requires a different level of processing. In the terminology of the present article, physical identity would be labeled as stimulus set condition and name identity as response set (again, this description is in relative rather than absolute terms). The former entails the use of preattentive mechanisms; the latter entails the use of focal attention. Posner and Mitchell also reported that within the name identity conditions, there were marked differences between the mean reaction time to respond "same" for the pair Cc and the mean reaction time to the pairs Aa, Bb, and Ee. According to the present theoretical framework, in the case of lower- and uppercase C, preattentive mechanisms can be employed (because of the similarity of both characters, and thus the identity can be judged on the basis of global physical features) even if the initial instructions are of name identity, which is response set in nature. An important implication of this result is that stimulus set is mainly determined by the nature of the environment of organismic factors like predispositions, expectations, and so on.

Sternberg (Note 2) developed a quantitative model of the search process, which assumed the same mechanism for both the visual search and item recognition tasks. The model is based on a serial comparison process with an exhaustive memory scan coupled with a self-terminating visual scan. Each item in the visual display is exhaustively compared, in turn, with the items in the memory set. The model predicts that reaction time in these tasks is an increasing linear function of the number of items in the display. Though not mentioned by Sternberg, the model is clearly restricted to response set conditions. Cavanagh and Chase (1971) have erroneously used a Sternberg-type paradigm to compare Sternberg's model and Neisser's preattentive mechanisms. They concluded that "A comparison of a forced choice visual search task, with an item recognition task did not support Neisser's (1967) hypothesis of a preattentive stage that processes targets and nontargets differentially" (Cavanagh & Chase, 1971, p. 493). A close look reveals, however, that response set rather than a stimulus set situation was investigated in this study. There were no external physical features on which preattentive mechanisms could operate; in addition, the size of a memorized set of possible targets was a main factor manipulated in the experiment, which is another indication that the experimental paradigm used was inappropriate to study preattentive mechanisms. On the other hand, the work of Beck (Beck, 1972; Beck & Ambler, 1972), who was careful to select only stimulus set conditions, strongly supports the notion of preattentive mechanisms (see also Kahneman, 1973).

A complete review of the relevant literature and the methodological implications suggested by the theoretical framework that was proposed here, is beyond the scope of this article. The few examples that were reviewed suffice to support the final conclusion: Experimenters should sharply distinguish between stimulus and response set situations and should be aware of any confounding of these two conditions. Overlooking the response–stimulus set classification will result in further confusion and erroneous conclusions. In this respect, the present framework adopts a Brunswikian point of view (see Hammond, 1966), which argues that a necessary prerequisite for any psychological research is to study and understand the nature of the environment with which the organism interacts.

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Adequate and Inadequate Stimulus Information: Comments on "Some Considerations of Two Alleged Kinds of Selective Attention" by Keren

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In this commentary I want especially to discuss the rationale behind the *before–after* experimental paradigm used by Keren (1976) in his Experiment 1. In this paradigm, a larger number of stimuli is presented than the subject must report on; this larger set of stimuli is shortened to a smaller subset by instructing the subject as to what the smaller set of stimuli is to which he is to respond. This instruction may be given before or after the actual presentation of the full set of stimuli.

Following Broadbent (1970), Keren refers to two kinds of instruction: In one instruction (producing stimulus set), the subset is specified by naming some stimulus characteristic, such as red or black; in the other instruction (producing response set), the subset is specified by some characteristic that is not readily identified as a simple stimulus property, such as requiring report of numerals or letters. Both Keren and Broadbent (and many other writers on this topic as well) have been aware that there is no possible way of specifying a class of responses in which there are, in fact, no stimulus differences between the stimuli to be responded to and those to be ignored. It is this point I want to emphasize as well as to show that by making some stronger assumptions, the *before–after* experimental paradigm can be used to ask questions about stimulus properties and their psychological utility.

The assumption that some instructions produce only response set is a definition by default; it is asserting that if there is no usable stimulus information contained in the instructions, then the instructions are producing only a limitation of responses rather than an input limitation. In other words, stimulus set (to quote Broadbent, 1970, p. 54) "would make it helpful to reject irrelevant stimuli at the time of arrival," while response set would not. It is this assumption that leads to the prediction of a large *before–after* difference with stimulus set and a low or zero difference with response set. But this is really a prediction, in positive form, only about the usefulness of stimulus properties and makes no positive prediction about the usefulness of response instructions at all. This whole approach is simply asserting that response instructions are useless because they give no useful stimulus information. Thus, the true experimental question is whether a particular instruction gives adequate or inadequate stimulus information, not whether it gives stimulus or response information.

But if this is so, then much stronger predictions can be made than usually are. Table 1 shows an idealized set of data (with numbers in the range of those found by Keren in his Experiment 1). These numbers are completely consistent with (a) the idea that the difference between the two types of instruction is related to the adequacy of the stimulus information contained in the instructions and (b) with Broadbent's assumption that the utility of the instructions lies in their ability to allow rejection of irrelevant stimuli at the time of input. Table 1 predicts, as did Keren, that there should be a difference in performance in the *before–after* comparison for adequate stimulus in-

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TABLE 1

An Idealized Set of Percent Correct Data for the *Before–After* Experiment

Instruction	Type of stimulus information	
	Adequate	Inadequate
Before	80	40
After	40	40

formation and no difference for inadequate stimulus information; it also makes the prediction that there should be no difference in performance between adequate and inadequate stimulus information in the *after* condition.

Now consider Table 2, which shows Keren's data (rounded off) for the separated stimuli (i.e., stimuli of the same stimulus or response set that are not adjacent in the stimulus display). In contrast to Table 1, how should these data be interpreted? To me, the answer is that the intended adequate stimulus information clearly was adequate, giving much better performance in the *before* than in the *after* condition. Thus we know that the specification of color is indeed a provision of adequate stimulus information. But what about the intended inadequate stimulus information? It clearly was less adequate than the intended adequate information, but it was far from inadequate. It is obvious from these results, then, that specification of numerals rather than letters (or vice versa) is an instruction that provides stimulus information, although less than is provided by specification of the color of the stimuli. This is not a conclusion that should be surprising: The letters used in Keren's Experiment 1 were the capitals A, F, K, and N; the numerals were 2, 4, 5, and 8. All of the letters have only straight lines, while three of the four numerals have curved lines. Thus, there is indeed a great deal of stimulus information in this so-called response set, but the information is less useful than that in the color difference between red and black.

Now we can ask an additional question, namely, whether information in the stimuli that is redundant to that specified in the instructions (although not itself specified in the instructions) can be used. The redundant information is spatial, all stimuli of the correct class being adjacent. Table 3 shows Keren's data for adjacent stimuli in Experiment 1 (also rounded off). What conclusions can be drawn from these data?

First, consider performance when the instructions are given before. Redundant spatial adjacency adds little to the effectiveness of the intended adequate stimulus information, since performance rises only from 78% to 85%. However, it adds a great deal to the intended inadequate stimulus information, raising performance from 67% to 82%, a performance that is not significantly different from that with the adequate stimulus information. Thus the redundant information is useful, but primarily when the need for it is great, as with the inadequate stimulus information—not a truly new conclusion as a general principle, but a new one with regard to these particular stimulus properties. When the instructions are given afterwards, performance rises for both adequate and inadequate stimulus information conditions, suggesting that even in the *after* condition additional stimulus information can provide some facilitation.

Altogether, these results suggest that it would be profitable to change the problem from one of asking about stimulus versus response to asking about the adequacy of different stimulus properties. The tempta-

TABLE 2

Keren's Data for Separated Stimulus Elements in Experiment 1

Instruction	Type of stimulus information	
	Adequate	Inadequate
Before	78	67
After	36	43

TABLE 3

Keren's Data for Adjacent Stimulus Elements in Experiment 1

Instruction	Type of stimulus information	
	Adequate	Inadequate
Before	85	82
After	47	57

tion to quote myself from some years back is too great to resist: In 1962 while discussing this very problem, I said, "However, to really find out about the nature of this perceptual effect, we will have to do experiments which intentionally control the *nature* of the restriction, not just the amount" (Garner, 1962, p. 50). And later (pp. 132–134), I suggested that multidimensional stimulus differences between the appropriate and the inappropriate stimulus subsets would be less valuable than single-dimensional differences. Certainly color differences are unidimensional, although the difference between letters and numerals is multidimensional. Thus the experimental result obtained by Keren allows the conclusion that single-dimensional differences are more effective than multidimensional differences, providing, in other words, more adequate stimulus information.

What About Response Information?

If pure response sets cannot be provided because there is always confounded stimulus differentiation, is it impossible ever to ask questions about the effectiveness of response information? In a strict logical sense, the answer has to be that it is not. But as a practical matter, and with a sufficient number of converging experiments, certainly questions can be asked about the adequacy of response information. While I do not want to address this large problem here, there is one point that is rather necessary to raise because of the assumption made in the ideal case portrayed in Table 1. That assumption is that response information given in advance is not useful information because it fails to allow stimulus selection. In fact, the entire rationale behind Keren's Experiment 1 is that there should be no *before–after* difference with what he calls *response set* and what I have called *inadequate stimulus information.*

Is the assumption that advance response information is not more valuable than post response information valid? The data presented here suggest that if stimulus information were inadequate enough, then response information given either before or after would provide no better performance than stimulus information given after. However, it might be well to note that the response information given restricts the number of responses to four, and it might be that further restriction would show an advantage to advance information.

A reason for suggesting this possibility lies in some preliminary and unpublished results of Donna Sutliff's (Note 1) doctoral dissertation research at Yale University. She uses a probabilistic cue in advance of a single-stimulus presentation, the stimuli being either good or poor patterns. The cue can be for pattern goodness, itself, or for one of the two permissible responses. The cue is given 1 sec in advance of the stimulus; with this much time, response cuing appears to be just as effective as stimulus cuing. There are no *before–after* comparisons in her experiments, but her data do suggest that response cuing in advance can be valuable. So the assumption that advance response information is not valuable may be weak, or at least greatly limited by the number of responses that are possible after response information is given.

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Short-Term Retention of the Where and When of Pictures and Words

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SUMMARY

Do true qualitative differences exist between the processing of pictorial and linguistic materials or are the differences simply a matter of degree? The dual code hypothesis (Paivio, 1971) assumes the existence of verbal and imaginal coding systems. The former is specialized for the coding of linguistic and sequential information, while the latter is specialized for pictorial and spatial information processing; it is also most durable. Alternatively, Anderson and Bower (1973) postulate a unitary processing system where all information enters a conceptual data base. While the unitary model appears to predict only quantitative performance differences, the dual code hypothesis predicts both qualitative and quantitative effects.

Because both models predict quantitative differences, such effects are not sufficient to distinguish between the two models. Furthermore, quantitative effects, as investigated most often, are difficult to interpret, since it is virtually impossible to equate words and pictures on most psychological dimensions. That is, picture superiority effects are ambiguous simply because the level of interitem similarity across form classes has not been equated. Consideration of these difficulties led to the conclusion that direct comparisons of performance on pictures and words should not be attempted at this time. Rather, the pattern of results within form classes should be compared. If qualitative differences exist, the patterns across conditions should be different for the two types of materials; if only quantitative effects exist, the trends should be similar. Experiments using variants of this paradigm point to the potential existence of both qualitative and quantitative differences in the processing and retention of pictorial and linguistic materials.

The present experiments investigate the retention of temporal and spatial structures as a function of form class. The dual code hypothesis predicts that, with words, temporal structures should be retained better than spatial structures; with pictures, the reverse should hold. The unitary model predicts similar trends for the two form classes. A series of experiments demonstrate that (a) temporal and spatial structures may be coded independently of one another, (b) linguistic materials lead to temporal superiority whereas pictorial forms give rise to temporal/spatial equality, (c) imposed encoding strategies do not influence the above patterns, and (d) imaginal processing does not necessarily lead to superior retention. These findings support the basic spirit, though not all of the particulars, of the dual code hypothesis and raise several questions concerning the nature of temporal and spatial structures.

In recent years many studies have been performed with the intention of comparing the processing of linguistic events with the processing of pictorial events. The objective of these comparisons has been to determine whether the processes operating on the two form classes of stimuli are similar to or are different from one another. Two theoretical stances are prominent today. Paivio (1971) has been the leading proponent of the dual coding hypothesis, which assumes two distinct yet richly interconnected encoding systems. The verbal system is said to be characterized by a propensity for serial processing and the encoding of sequential information. The imaginal system is specialized for the processing and representation of spatial information. Typically, linguistic stimuli would be subject to verbal encoding processes though if the items are concrete, and/or if the situation warrants it, imaginal processes may be engaged. On the other hand, while pictorial stimuli are always subject to imaginal processing, verbal processing may also occur, particularly when the pictures are readily named. Hence, the internal memory representations of the various form classes may range from predominately verbal to predominately imaginal. Furthermore, since imaginal coding is presumed to result in a more durable memory representation than verbal coding, any situation that leads to extensive use of imaginal processing should lead to superior retention (Paivio & Csapo, 1973).

Several other theorists (Anderson & Bower, 1973; Chase & Clark, 1972; Pylyshyn, 1973) have argued for a unitary form of representation. Regardless of form class, they suggest that all information is interpreted into a single conceptual–propositional data base. In this view, differences in the encoding of pictures and words arise because the representation of pictures is potentially richer in detail than the representation for words. The unitary theorists have had little to say about the initial processing routines applied to pictures and words, though one might expect some differences to exist. However, the essential point is that while the dual code hypothesis predicts qualitative as well as quantitative differences in the internal representations, the unitary theorists predict primarily quantitative differences.

The research reported here sought first to distinguish between these two theoretical stances by using tasks that, according to the dual code hypothesis, ought to demonstrate specific qualitative differences in the processing and, hence, retention of words and pictures. A second purpose was to point out some of the difficulties involved in making valid picture/word comparisons and to suggest some methodological strategies that may overcome some of these difficulties. To that end, a selective review of the existing literature follows, along with methodological commentary.

The purported superior recall and recognition of pictorial materials (Dallett & Wilcox, 1968; Shepard, 1967; Standing, Conezio, & Haber, 1970) and the positive effects of the use of imagery strategies (cf. Paivio, 1971) have been used to support the dual code hypothesis. Yet, these effects are purely quantitative and could be equally well explained by unitary theories. It should also be noted that recognition levels can be reduced dramatically simply by increasing interitem similarity (Goldstein & Chance, 1970). Indeed, until performance on sets of equally complex and discriminable pictorial and linguistic forms is compared, little can be said concerning the superiority of picture recognition.

However, until much more is known about the critical aspects of pictures, it is impossible to equate pictures and words such that possible sources of confounding are eliminated. For example, Wells (1972) at-

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tempted to equate pictures and words by decomposing a simple picture into its component parts. While performance differences were found in her experiments, interpretation of those differences is difficult, for it is not clear that the decomposition process achieved its purpose. That is, decomposition may have produced equivalent stimuli in some sense, or it may have "destroyed" the essence of the picture, or it may have introduced some unknown type of confounding. In general, our problem is that we don't know enough about pictures per se.

Given our current state of ignorance, it may be that a direct comparison of performance on pictures and words is not warranted. It is not only difficult to interpret the outcome of such direct comparisons, but the interpretation typically does little to further our understanding of the operations involved in the processing and retention of such stimuli. That is, very little information concerning potential qualitative effects is revealed by such comparisons. An alternative technique might be one in which trends within the form classes are compared. If true qualitative differences do exist, it should be possible to find two conditions, A and B, where memory performance on words is better in Condition A than in Condition B. For pictures, another pattern should be observed. Assume, for example, that Conditions A and B have been found that produce a complete crossover interaction between form class and the conditions, with a main effect of pictorial superiority. Does this pattern of results guarantee that qualitative differences are being tapped? Not absolutely, for one could interpret the trends in terms of differential processing efficiency. However, the onus would be on the quantitative theorist to explain why processing efficiency should vary across the various conditions as a function of form class. Note also that the presence of a quantitative word/picture main effect would influence interpretation only if it produced potential floor or ceiling effects. Thus, the primary advantage of this approach is that qualitative differences between pictures and words can be investigated relatively independently of quantitative differences in base performance levels.

Experiments utilizing the above approach fall into three main classes dealing with (a) the effects of variation of rehearsal time on retention, (b) selective interference of verbal and pictorial codes, and (c) lateralization of verbal and nonverbal functions. In the rehearsal experiments, Conditions A and B would refer to more or less rehearsal time. Typically, retention of linguistic material increases systematically with increased rehearsal time. For pictorial materials, the trend is uncertain. Hintzman and Rogers (1973), Shaffer and Shiffrin (1972), and Potter and Levy (1969) were uniformly unsuccessful in demonstrating rehearsal effects beyond one item. On the other hand, Tversky and Sherman (1975) and Weaver (1974) did find evidence indicating that retention of pictorial materials improved as a function of increased rehearsal time. Because the experimental procedures and materials were quite different across these five studies, it is difficult to specify the necessary conditions that lead to different rehearsal capabilities for words and pictures. Though suggestive that some processing differences may exist between pictures and words, the rehearsal experiments thus far remain mute as to the nature of these differences.

In the case of selective interference paradigms, Conditions A and B refer to different types of distracting tasks. For example, Pellegrino, Siegel, and Dhawan (1975) compared the short-term retention of words and pictures following either an auditory–verbal task, a visual nonverbal task, or both. The auditory task selectively interfered with word retention, while the auditory-plus-visual task selectively interfered with picture retention. No selective interference effects were noted following the visual task. These results seem to suggest that under recall instructions, at least, pictorial materials are encoded into both verbal and imaginal stores, whereas linguistic materials are encoded primarily into a verbal store. Several other experimenters have demonstrated selective interference of either verbal item or spatial information, depending upon the nature of the intervening task (den Heyer & Barrett, 1971; Meudell, 1972; Salthouse, 1975). While these selective interference experiments suggest that qualitative differences in the processing and retention of pictorial (spatial) and linguistic materials do exist, the specific nature of these differences has not yet been revealed.

In the lateralization of function research, Conditions A and B refer to the presentation of material to the right or left hemispheres. Although hemispheric lateralization of linguistic and nonlinguistic functions has been well documented in pathological cases (Gazzaniga & Sperry, 1967; Milner, 1971), the effects are frequently small, sometimes difficult to reproduce, and often quite complex when intact individuals are tested (Broadbent, 1974). Concerning other pathologies, verbal aphasias and agnosias are frequently that—verbal. Nonlinguistic processes seem to function quite well. On the other hand, there are three reported cases of visual agnosia where the patient can read, but has difficulty identifying objects and complex visual arrays other than the printed word (Geshwind, 1969). For a detailed case study of one such individual, see Taylor and Warrington (1971). Again, these results and observations lend credence to the notion that qualitative differences exist. They also hint at the potential level of complexity of these differences.

As noted before, while much of the research supports a qualitative model in general, little is known about the specific qualitative differences. The present experiments attempted to document one possible difference: the efficiency in the processing and retention of temporal and spatial information. If, as the dual code hypothesis suggests, the verbal code is especially sensitive to sequential order and the imaginal code is specialized for representing spatial relations, then memory for temporal order and spatial position should vary according to the type of stimuli used. That is, for words, memory for temporal order should be superior to memory for spatial position; for pictures, the reverse should be true. While inspection of a picture proceeds in a serial fashion (fixation by fixation), the relationships that are perceived and retained are spatial–structural ones. Hence, the processing and retention of spatial relations is likely to be more salient than the processing and retention of temporal order of input. On the other hand, processing of linguistic structures (e.g., reading, speaking, and hearing) seems to be quite dependent upon the sequential properties of the stimuli (regarding the effects of letter-transition probabilities in word recognition and the effects of syntax on comprehension and retention, cf. Neisser, 1967; Paivio, 1971). Thus, for linguistic stimuli, the processing and retention of sequential information may be most salient.

Before considering evidence for the hypothesis, the relationship between temporal and spatial structures should be considered. Whether spatial information is independent of (Mandler & Anderson, 1971) or derived from (Murdock, 1969) temporal structures is unclear. Mandler and Anderson were able to obtain estimates of performance when only temporal or spatial cues were used in recall. The joint action of these cues, assuming independence, closely predicted recall performance on structures where temporal and spatial cues were perfectly correlated. The authors suggest that "*S*s may use temporal and spatial structures as independent and additive retrieval cues" (Mandler & Anderson, 1971, p. 128). Murdock, on the other hand, analyzed the distribution of the magnitude of errors. He found that the error gradients for spatial recall were flat, whereas for temporal recall, proportionally more errors were of small magnitude than of large magnitude. From this evidence, Murdock (1969) suggested that "spatial information is extracted through a temporally mediated reconstruction" (p. 383). The present Experiment 1 was designed so that both types of analyses could be performed in an attempt to resolve these conflicts.

Keeping in mind the ambiguity of the nature of temporal and spatial structures, let us review the existing evidence that bears on the relationship between words and pictures and between temporal and spatial structures. Paivio and Csapo (1969, 1971) have shown that the reproduction accuracy of the sequential (temporal) order of input was better for words than for pictures when stimuli were presented at a rate sufficiently fast to prevent implicit naming of the pictures (5.3 items/sec). At slower rates, reproduction accuracy was superior for pictures. However, in Paivio and Csapo's experiments, accuracy levels in the former condition were precariously close to chance levels, and it is quite possible that the demanding input conditions resulted in peculiar processing strategies. Superior discrimination of relative recency was found with pictorial materials by Fozard (1970), leading him to suggest that temporal tagging was most efficient when pictorial materials were used. Rothkopf (1971) tested incidental memory for the location of information within and across pages of a 3,000-word text and found greater than chance accuracy. Under both intentional and incidental testing, Schulman (1973) demonstrated that the location of a printed word was recognized between 30% and 35% of the time (chance = 25%). Mandler and Stein (1974) and Mandler and Parker (1976) found that spatial relations were retained extremely well when organized pictures were used, whether tested by recognition or reproduction. Via extrapolation, the above studies suggest that both temporal and spatial retention may be highest for pictorial materials. However, this apparent superiority may simply be another case of "pictorial superiority" in general and, as such, is subject to criticisms previously brought to bear on the making of direct comparisons between pictures and words.

Using only linguistic stimuli, Mandler and Anderson (1971) demonstrated that information concerning temporal structures was retained better than information about spatial structures under a variety of conditions. When Healy (1975b) used a procedure similar to that used by Mandler and Anderson, temporal superiority was observed when the stimuli were consonant strings; modifications of the procedure (to be discussed later) led to temporal/spatial equality (Healy, 1975a). Evidence related to temporal/spatial patterns observed with pictorial–imaginal stimuli is very indirect or contradictory. Error data suggest that the deaf have a tendency to use a visual–kinesthetic code (Bellugi & Siple, 1973). Experiments by O'Connor and Hermelin (1973) and Hermelin and O'Connor (1973) demonstrated that an ambiguous display tends to be encoded spatially by deaf children, whereas the same displays are almost always encoded temporally by hearing children. If the hypothesized visual code of the deaf is related to the imaginal code proposed by Paivio (1971), it may be reasonable to expect that the hearing subject would be most sensitive to spatial information when the imaginal system is activated by the use of pictorial materials.

Only one experiment has been reported that compared the retention of temporal and spatial information as a function of the type of stimuli. The results were contrary to both the predictions of the dual code hypothesis and the evidence advanced above. Snodgrass and Antone (1974) found that the spatial relationship between pairs of items was recognized better than the temporal sequencing of the pair members. This effect was found for both words and pictures, and there was no hint of an interaction. Such results are clearly supportive of a unitary model that predicts only quantitative effects.

Overview of the Experiments

All experiments were variations on the basic methodology reported by Mandler and Anderson (1971). Eight items were presented one-by-one in a horizontal array. Typically, temporal order of presentation was uncorrelated with the spatial position of the items. Following a single presentation, the person attempted to reproduce either the temporal or spatial structure of that trial. Each person received an equal number of temporal and spatial reconstruction trials in a single session. In various experiments, stimuli were common nouns, line drawings of common objects, or line drawings of complex three-dimensional geometric figures.

To eliminate the necessity for verbal encoding, a reproduction task was used. This task was completely self-paced, and people were free to rearrange items until they were satisfied with the resultant ordering. Thus, testing conditions allowed people to utilize all information at their disposal to create a correct ordering.

The presentation rate of the items was

relatively slow (1.5 sec/item with a .7-sec interstimulus interval). Pilot investigations revealed that this rate resulted in accuracy levels that were unconfounded by either floor or ceiling effects. At such a slow rate, pictures (especially those of common objects) could easily be named. While it would be advantageous to eliminate this possibility, the problems posed by using very fast presentation rates were deemed more serious than the naming problem. Various attempts were made to control for naming strategies, as will be explained later.

EXPERIMENT 1

Experiment 1 considered two issues. First, it sought to determine whether temporal and spatial structures are independent of one another, and whether this relationship varies as a function of the form class of the stimuli. For words, the existing evidence is contradictory; for pictures, no data exist. The three most logical possibilities are that: (a) the structures are independent for both form classes; (b) spatial structures are derived from temporal structures for words, and temporal structures are derived from spatial structures for pictures; or (c) spatial structures are derived from temporal structures for both types of stimuli. Independence of the two structures seems most likely, as it appears that people can remember the when and not the where (or vice versa) of events in many modalities. The second possibility would support the dual code theory, whereas the third possibility would render this paradigm useless in the search for qualitative differences in the representations of words and pictures.

The second issue, of course, concerned the nature of the relationship between memory for these structures and the form class of the stimuli. According to the dual code hypothesis, memory for temporal structures should be superior to memory for spatial structures when the stimuli are words and vice versa when the stimuli are pictures. Quantitative theorists would predict the same pattern of results for both form classes; from the Snodgrass and Antone (1974) data, spatial superiority should be observed for both types of stimuli.

Stimuli were either common nouns or line drawings of common objects. To encourage the use of one encoding strategy, hopefully appropriate to the stimulus, this was a between-subjects factor. Temporal order of presentation was either correlated or uncorrelated with the spatial position of the items. Thus, there were three types of reconstruction task: temporal, spatial, and a one-to-one task, where temporal and spatial structures were identical. To control for the effects of individual differences, type of task was a within-subjects factor. The inclusion of the one-to-one task allowed a test of the suggestion that temporal and spatial cues function as independent and additive components in a serial task. That analysis, together with an analysis of the distribution of the magnitude of errors, was used to assess the nature of the relationship between temporal and spatial structures. Another variable investigated was the effect of cuing. Subjects were either cued before or after the presentation as to which structures they would have to reproduce. Postcuing was expected to lower accuracy levels. Moreover, it was hoped that under the more difficult condition of postcuing, the proposed compatibility between temporal order and words and between spatial position and pictures would be exaggerated. Finally, serial position analyses were used to localize the source of whatever differences emerged.

Method

Design. The design included stimuli (words or line drawings) and cuing (pre- or postcuing of type of reconstruction task) as between-subjects factors. Within-subjects factors were reconstruction task (temporal order, spatial position, or the one-to-one task), halves (first or second half of the session), and serial position (Serial Positions 1–8 for temporal order and spatial position). The eight subjects in each Stimulus × Cuing condition were given five trials on each of the reconstruction tasks in a random order, during each half of the session.

Spatial position referred to the place on the wall where the item was presented, and temporal order was defined by the temporal order of input. Twenty different input orders were generated such that the correlation between temporal order and spatial position was minimal and no obvious temporal/spatial patterns were discernible. A typical input order for one trial might be the following: 7 2 8 5 1 4 6 3. The value of the number refers to the temporal order of input, and the position of the number from left to right refers to the items' spatial positions. Thus, in this example, the first item presented occurred in Spatial Position 5, the second temporal item occurred in Spatial Position 2, and so on. Correct reconstruction of spatial position would be 7 2 8 5 1 4 6 3; for temporal reconstruction, the proper sequence would be 1 2 3 4 5 6 7 8. For spatial reconstructions, subjects laid the items out in a horizontal array, while for temporal reconstruction, a vertical array was used.

On the one-to-one trials, stimuli were always presented from left to right in order. Hence, temporal order and spatial position were perfectly correlated. On half of the one-to-one trials, subjects were told to pay attention to the temporal order, and on half they were told to pay attention to the spatial position.

For temporal order reconstructions, serial position was simply the temporal order of input. For spatial position reconstructions, serial position went from left to right, according to where the items were presented.

Subjects. The subjects were 32 volunteers from the Toronto subject pool. Subjects were assigned to one of the four experimental conditions according to a prearranged random order.

Stimuli. The 64 line drawings were simple black and white outline drawings with a moderate amount of detail of common objects such as a flower, table, bird, and knife. All were readily "named" by a number of randomly chosen colleagues. The 64 words were chosen from the Toronto word pool, and all denoted common concrete objects or places. They were two-syllable nouns with a frequency count of 20 per million or more according to the Thorndike–Lorge (1944) word count.

All stimuli were made into 35-mm slides for presentation purposes; for the reconstruction task, Xerox copies of the line drawings were mounted on 7.5-cm square cards, and the word stimuli were printed on similar cards.

From each pool of 64 stimulus items, 8 test lists, all of List Length 8, were generated by sampling without replacement from each pool. This procedure was repeated three additional times, yielding 4 sets of 8 lists. Items within a list were assigned a spatial position such that each repetition of the item in each of the other 3 sets occurred in different locations. Two presentation orders (A and B) of 15 lists were created by combining 2 sets and dropping 1 of the 16 lists. For each of these orders, 2 random reconstruction task assignments (5 trials each of the temporal, spatial, and one-to-one tasks) were generated; hence, there were 4 basic presentation orders (A, A′, B, and B′). Item input orders (described above) were then assigned to the temporal and spatial tasks.

Each subject was tested on one order, that is, 15 lists, in each half of the session. For the first four subjects in each condition, the order of testing was as follows: AB, A′B′, BA, and B′A′. For the remaining four subjects, the temporal and spatial task assignments were interchanged, and the basic order of testing was repeated.

Apparatus. Eight Leitz Color Prodovit projectors were positioned on a scaffolding 5 m from the floor, so as to form a horizontal array. They were aimed at eight positions on the wall; each position was marked by a strip of black tape that corresponded to the bottom edge of the projected image. Projection width was approximately 2.3 m, and each projected image occupied an area 20-cm square with a 10-cm separation. A series of relays connected to each projector, a pair of Hunter Clock Timers, and eight telegraph keys allowed the experimenter to activate one projector after another in the appropriate presentation order at a controlled rate.

Procedure. Subjects were tested individually in one 45-min. session. Throughout the session, the subject was seated at a table approximately 1.5 m from the projection wall and centered with respect to the projection width. The experimenter was seated to the left somewhat behind the subject. On the table were two arrays, one vertical and one horizontal, to be used in the reconstruction task.

Each subject was given extensive instructions concerning the task, followed by four practice trials with complete feedback, two each of the spatial and temporal tasks in alternation. The practice stimuli for the line drawing groups were simple geometric shapes; for the word groups, one-syllable words were used. In the precued groups, all practice trials were precued. In the postcued groups, the first two trials were precued and the last two were postcued. If a subject seemed confused at the end of the practice, four more practice trials were given. This occurred for only one subject.

A typical trial in the precued conditions proceeded as follows. The subject was told to pay attention to either the temporal order or the spatial position of the items. One second after a spoken *ready* signal, the eight items were presented one by one according to the prescribed input order for that trial. Each item appeared for 1.5 sec, and there was a .7-sec interstimulus interval. After the eighth item had been presented, the subject was handed a randomized deck containing the items he or she had just seen. For temporal reconstructions, the subject used the vertical array, and the horizontal array was used for the spatial reconstruction. For the one-to-one input order, the subject used either the horizontal or vertical array according to the temporal or spatial instruction. The reconstruction task was totally unstructured, in that the subject was free to rearrange the items until he or she was satisfied. After the experimenter recorded the reconstruction and collected the cards, the next trial was initiated. No feedback was given in the experimental trials.

The only difference between the precued and the postcued conditions was when the subject received the temporal or the spatial instruction. In the precued case, the subject was informed immediately prior to the beginning of the presentation. In the postcued case, the instruction was given after the subject had seen the last item in the sequence.

Results

Analyses pertinent to the question of qualitative differences between pictures and words will be presented first, then analyses pertinent to the relationship between temporal and spatial structures. A strict criterion was used to score the reconstruction protocols. Only items placed absolutely accurately were considered as correct. The number of correct placements for each serial position summed over the five replication trials for each subject in each half of the session served as the data base. These totals were submitted to an analysis of variance for repeated measures. Stimulus and cuing were between-subjects factors; task, halves, and serial position were within-subjects factors. Two analyses were performed using this design: In one, data from all three reconstruction tasks (temporal, spatial, and one-to-one) were used; in the other, only the temporal and spatial reconstruction data were included in the analysis. Since the results of the two analyses were virtually identical, only the first analysis will be considered. Unless otherwise noted, all relevant F values were significant at the .05 level of confidence or beyond.

Before one-to-one data could be included in the analysis, it was necessary to determine if the different instructions had led to differential performance. One half the one-to-one trials, subjects were instructed to pay attention to temporal order; on the other half, they were instructed to attend to spatial position. Under temporal order instructions, a mean of 5.8 items were correctly placed; under spatial position instructions, the mean was 5.9 items. Apparently, different instructions did not affect one-to-one performance, and hence, no further distinction between the types of one-to-one trials will be made.

TABLE 1

Mean (M) Proportion of Correct Temporal and Spatial Placements as a Function of Type of Stimulus and Cuing Instruction in Experiment 1

Condition	Reconstruction task			M
	Temporal	Spatial	One to One	
Stimulus				
Line drawing	.69	.63	.78	.70
Word	.61	.45	.68	.58
Cuing				
Pre	.78	.60	.79	.72
Post	.52	.49	.66	.56
Task M	.66	.54	.73	

Placement accuracy, expressed as a proportion, is shown in Table 1 for the three main factors of stimulus, cuing, and task. As can be seen, accuracy of placement was highest for line drawings, $F(1, 28) = 17.09$, and for precued conditions, $F(1, 28) = 31.30$, $MS_e = 8.41$. Reconstruction performance was best for the one-to-one task and poorest for the spatial task overall, $F(2, 56) = 37.19$, $MSE_e = 3.04$.

While the Stimulus × Task interaction did not reach an acceptable significance level, $F(2, 56) = 2.56$, $p < .07$, comparisons appropriate to the stated hypothesis were performed. Temporal reconstructions were significantly more accurate than spatial reconstructions for both line drawings and words. Also, one-to-one reconstructions were superior to temporal reconstructions for both types of stimuli.

The interaction of Cuing × Task was highly significant, $F(2, 56) = 6.49$, $MS_e = 3.04$. As can be seen from Table 1, the performance decrement resulting from postcuing was twice as large for the temporal task than for the other tasks. The nature of this loss was revealed by inspection of the serial position data. As expected, serial position was highly significant and entered into several interactions. The significant double interactions of Cuing × Serial Position and Task × Serial Position were qualified by a significant triple interaction involving all three factors, $F(14, 392) = 2.40$, $MS_e = .87$. This interaction is shown in Figure 1. The main effect of postcuing was to exaggerate the bowed character of the serial position curve by lowering performance on the middle positions, while leaving the ends virtually un-

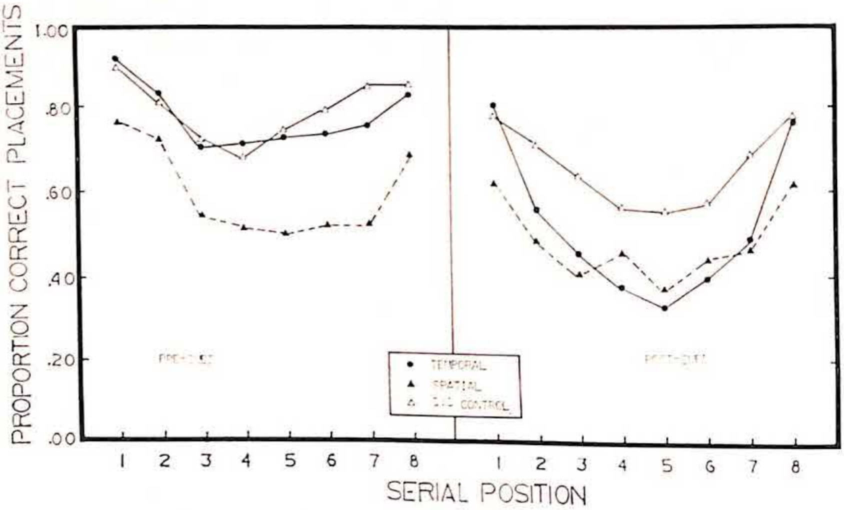

FIGURE 1. Temporal, spatial, and 1:1 reconstruction task performance as a function of serial position for pre- and postcued conditions in Experiment 1.

affected. This effect was most apparent for the temporal task. Under precued conditions, the temporal and one-to-one curves were almost identical; however, under postcued conditions, the temporal curve showed much greater losses than the one-to-one curve for all positions except the endpoints. A comparison of the temporal and spatial curves revealed similar shapes in the precued conditions. Under postcuing, the asymptotic performance levels were similar for temporal and spatial tasks, though the recency and primacy effects were larger for the temporal task.

Performance was significantly better in the second half of the session (.75 vs. .62), $F(1,\ 28) = 4.60$, $MS_e = 1.71$. The interaction of Halves × Serial Position, $F(7,\ 192) = 4.32$, $MS_e = .89$, revealed that the second half superiority was due to increased accuracy in Positions 3–7 in the second half of the session. Finally, the interaction of Task × Halves × Serial Position, $F(14,\ 392) = 2.25$, $MS_e = .91$, localized second half superiority to increased accuracy in the middle positions of the curve for only the temporal and one-to-one tasks. Performance on the spatial task remained relatively constant across both halves of the session.

One aspect of the relationship between temporal and spatial structures was examined by assessing the predictive power of the assumption that performance on one-to-one trials was a function of the joint action of independent knowledge of temporal and spatial structures. If the assumption of independence is valid, performance on one-to-one trials should be predicted by Equation 1:

$$p^*(1:1) = p(t) + p(s) - p(t)p(s), \quad (1)$$

where $p^*(1:1)$ is the predicted level of one-to-one performance and $p(t)$ and $p(s)$ are estimates of temporal and spatial knowledge, respectively. Given the nature of the reconstruction task used in Experiment 1, estimates of $p(t)$ and $p(s)$ from temporal and spatial trials, respectively, will be inflated. That is, whether a subject knows nothing, or knows four or even seven positions correctly, that subject will be expected to place one item correctly by chance alone. While the probability associated with this chance event will vary with the number of unknown positions (from .125 when no position information is known to 1.00 when the person remembers seven positions correctly), the expected number of correct placements due to chance alone is one.[1] This analysis leads to Equation 2, where the observed probabilities of temporal and spatial placements are reduced by .125, which is simply the probability increment associated with the expectation that one item will be placed correctly by chance alone. Since observed performance on one-to-one trials is inflated by the same amount, the constant (.125) must be added to the predicted performance levels when temporal and spatial estimates are corrected for chance:

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[1] Mathematically, this leads to the following equalities: $p_o = p_k + .125$, for $.0 \leqslant p_k \leqslant .875$; and $p_o = 1.0$, for $p_k > .875$; where p_o is the observed probability of a correct temporal or spatial placement, and p_k is the probability representative of a person's actual level of position knowledge.

$$p^*_c(1:1) = p(t) - .125 + p(s) - .125 \\ - [p(t) - .125][p(s) - .125] + .125. \quad (2)$$

The mean probability of a correct placement for the temporal and spatial tasks at the level of the Stimulus × Cuing × Halves × Serial Position interaction was used to predict one-to-one performance from Equation 2. This analysis revealed that the predictions overestimated by a mean of .091 probability units with a standard error of .014. The size of the error did not vary systematically as a function of any of the factors. A linear regression on the observed–predicted values for these 64 points accounted for a substantial proportion of the variance ($r^2 = .60$), with a slope of .992 and an intercept of .092. This relationship held over a wide range of available accuracy levels (.35–1.00).[2]

The second aspect of the relationship between temporal and spatial structures involved error distribution analyses. In this analysis the magnitude of each erroneous placement in all protocols was scored. For example, if the third item (either temporally or spatially) was placed in the eighth position, the magnitude of the error was of Distance 5. The smallest error would be of Distance 1; the largest, of Distance 7. The total number of errors at each distance was corrected for the number of opportunities, since an error of Distance 1 could occur in any of 14 ways, whereas an error of Distance 7 could only occur in 2 ways. The corrected distance errors for line drawings and words for the three types of tasks are plotted as probability distributions in Figure 2.

For both types of stimuli, the error gradients were steepest for the one-to-one task and shallowest for the spatial task. Error gradients for line drawings were consistently steeper than those for words. In particular, the spatial error gradient for words was almost flat (with the exception of the smallest error). Though not directly shown due to collapsing across cuing conditions, the steepness of the error gradients seemed to be positively related to the difficulty of the task, in that the steepest gradient was found for the easiest task (line drawings–one-to-one, precued) and the flattest gradient was found for the most difficult task (words–spatial, postcued).

Discussion

First, what conclusions can be drawn concerning the relationship between temporal and spatial structures? The analysis of error gradients revealed that as the task became more difficult, the error gradients started progressively lower and became flatter; this effect was most prominent for the spatial task. From this, one might con-

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[2] Originally, these independence notions were tested using Equation 1, and the results of those analyses were substantially the same as the findings reported above. After one of the reviewers suggested that a correction for guessing should be used, the above analysis was developed with the assistance of Michael Rabinowitz. The reviewer suggested still another correction for guessing that was based on a high-threshold model of memory for temporal and spatial structures. In the reviewer's model, observed probabilities, p_o, are submitted to the following correction for guessing before being entered into Equation 3: $\bar{p} = (p_o - .125)/(1.0 - .125)$. Using this model:

$$p^*_{cr}(1:1) = \bar{p}(t) + \bar{p}(s) - \bar{p}(t)\bar{p}(s) \\ + .125\{1.0 - [\bar{p}(t) + \bar{p}(s) - \bar{p}(t)\bar{p}(s)]\}, \quad (3)$$

performance on one-to-one trials [$p^*_{cr}(1:1)$] was overestimated by .065 probability units (standard error = .013). The results of the linear regression yielded a slope of .946 and an intercept of .104; the amount of variance accounted for was 62%. Because the correction for guessing used in Equation 2 takes the subject's level of knowledge into account when making the correction, we prefer it to the correction made in Equation 3. However, regardless of which correction for guessing is used, the basic facts remain the same. Performance on one-to-one trials is constantly overestimated, and the overestimates do not seem to vary systematically as a function of the level of accuracy.

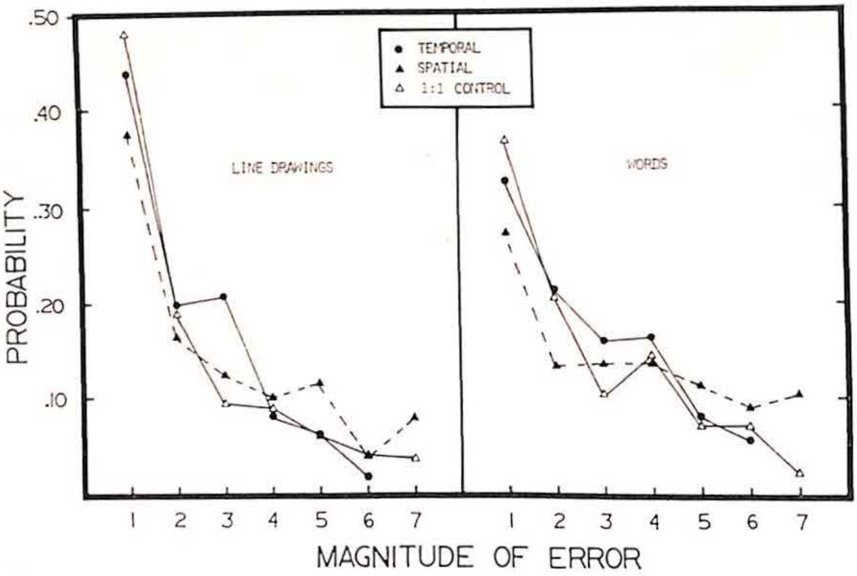

FIGURE 2. Probability distribution of the magnitudes of erroneous placements in the temporal, spatial, and 1:1 reconstruction tasks for the line drawings and words used in Experiment 1.

clude that temporal information may be easier to process directly in this task than spatial information, especially when the task is difficult and the stimuli are words. When the task is exceedingly difficult and words are used as in Murdock (1969), spatial information may indeed be reconstructed from temporal information. But, this need not be the case.

The notion that temporal and spatial cues function as independent and additive components in the one-to-one task received some support. That is, while all predictions were in error, the size of the error was relatively constant. To test the generality of this notion, two additional sets of available data were analyzed accordingly. The first set of data came from an unreported experiment in which complex geometric forms were used as stimuli; presentation and test procedures were identical to those used in the line drawing–precued condition of Experiment 1. Predictions, using Equation 2, were made at the level of Halves × Serial Position, and as in the previous analysis, predictions overestimated obtained values by approximately .10 probability units. The second set of data came from the third experiment reported by Mandler and Anderson (1971). In that experiment, people were given one study trial and then asked to recall the item that occurred between two probe words. On temporal trials, the probe words were the items that immediately preceded and followed the target item; on spatial trials, they were the two words on either side of the target item. The factorial combination of pre- versus postcuing and overt versus covert pronunciation of the items during the study phase in Mandler and Anderson's experiment resulted in four main input conditions suitable for analysis. Since their experiment required item recall rather than reconstruction, the estimates of temporal and spatial knowledge did not need to be corrected for guessing. Hence, Equation 1 was used to make predictions concerning one-to-one performance. Overall, Mandler and Anderson reported an overestimate of .06 probability units. When the data were reanalyzed at the level of Cuing × Pronunciation × Serial Position, the mean overestimate was .044. However, that value included an overestimation of approximately .11 probability units for three of the conditions and an underestimate of .17 probability units for the remaining condition.

The predictions from the two abovementioned additional experiments and from Experiment 1 were collapsed over serial position, and the means were entered into the observed–predicted linear regression analysis. The additional data fit quite well, with the exception of one condition. That is, the results of the linear regression on 13 means (excluding the condition in the Mandler and Anderson experiment that grossly underestimated one-to-one performance) revealed a slope of 1.087 and an intercept of .041; the amount of variance accounted for was 88%. In general, then, the overestimate appears to be quite consistent and seems to hold for both reproduction and probed-recall testing situations.

What is the source of the overestimation? Presumably the estimates of temporal and/or spatial information are inflated. Mandler and Anderson (1971) investigated the effects of the rank-order correlation of spatial and temporal information and found that partial correlation did not improve performance over zero correlation conditions. However, control over rank-order correlation does not insure elimination of a systematic relationship between temporal and spatial structures. For instance, the rank-order correlation between temporal and spatial structures for the sequence 8 6 1 2 3 4 5 7 is −.02. Yet, there is an obvious pattern relating the two structures. In both the present and Mandler and Anderson's experiments, such highly patterned sequences were rejected. However, the perception and use of any random pattern may be highly idiosyncratic. To the extent that individuals utilized patterning, estimates of temporal and spatial information in uncorrelated structures will be inflated. The only way to circumvent this situation appears to involve a between-subjects design, where one group is only tested on temporal and the other on spatial structures. In this situation, people would be less likely to attend to the irrelevant structure, and thus, "pure" estimates of temporal and spatial information may be obtained. As it rests now, it would appear that temporal and spatial structures in fact may act as independent and perhaps additive cues for serial recall, though the crucial data remain to be gathered.

Having shown that temporal and spatial structures are retained relatively independently, we can now proceed to consideration of the hypothesis that memory for temporal and spatial structures should vary as a function of the form class of the stimuli. Very simply, the hypothesis received very little support. For both types of stimuli, temporal structures were retained better than spatial structures, which is just the opposite of the results reported by Snodgrass and Antone (1974). While there were many procedural differences between the experiments, one seems crucial to resolution of the contradiction. In their experiment, members of the spatial pairs were presented simultaneously, side by side. Since people in Western society typically scan from left to right, the possibility exists that their spatial condition actually led to both temporal and spatial processing. The temporal pair items were presented sequentially in the same position and, hence, were subject to temporal processing only. Since temporal and spatial cues may be additive, it is not surprising that "spatial superiority" was observed in their experiment. In the present and the Mandler and Anderson experiments, the possibility of dual temporal and spatial coding was controlled. When only single codes were available, temporal superiority emerged.

One possible explanation for temporal superiority in the present experiment resides in the nature of the rehearsal strategies used to retain the structures. When people were asked about the strategies they used during the experiment, they invariably reported that it was quite easy to develop an effective rehearsal strategy for temporal structures, but that they were generally unsuccessful in creating a rehearsal strategy for spatial position. The interaction of task and cuing is supportive of these introspections. In precued conditions, all available time could be devoted to rehearsing the appropriate structures, whereas in the postcued conditions, rehearsal time had to be divided between the two structures. If rehearsal of spatial information is relatively inefficient or difficult, the amount of time available for rehearsal should have a small effect. On the other hand, if temporal information is easy to rehearse, variation in rehearsal time ought to produce a large effect. In fact, the effect of cuing on temporal accuracy was twice as large as the effect on spatial accuracy. Thus, temporal structures appear to have a processing advantage overall—perhaps because the structure is given directly by the presentation order—while spatial structures must be constructed.

Since the stimulus factor did not enter into an interaction with task and cuing, one might expect that people treated the words and line drawings in a similar fashion. On the whole, people reported rehearsing the names or even the first letter of the name of the line drawing. At the level of rehearsal, then, the two types of stimuli were treated similarly. These two strategy effects, relative ease of temporal rehearsal and the use of a name code, could be responsible for the obtained superiority of temporal structures for both words and pictures.

While the interaction between stimulus and task was not significant, it was true that the difference between memory for temporal and spatial structures was 2.5 times larger for words than for pictures. Can this trend be indicative of potential qualitative effects that were reduced by the two strategy effects noted above? Possibly, but this trend could equally well reflect differential forgetting rates for words and pictures and temporal and spatial structures. That is, assume that item memory decays more slowly for pictures than for words and that the decay rate for spatial is faster than that for temporal information. Given these assumptions, at time *t,* pictures would be remembered better than words, and the difference between the temporal and spatial forgetting curves would be smaller for pictures than for words. In fact, people frequently insisted that a particular test word hadn't been presented; this rarely occurred with the line drawings. Thus, the results of Experiment 1 can easily be interpreted by purely quantitative differences in efficiency and ease of processing without recourse to notions of qualitative differences.

Experiment 2 utilized a recognition–reproduction procedure to measure item retention as well as reproduction accuracy. Presentation conditions remained the same as in Experiment 1. At reconstruction, however, people were handed a set of 16 items that were a random mixing of the 8 target items plus 8 lures. Their task was to go through the deck, one card at a time. Each item, which was recognized as *old,* was to be placed in its appropriate location. If the person failed to recognize all 8 items, he or she was forced to select additional items until all 8 positions had been filled.

Aside from allowing measurement of item recognition, it was hoped that this procedure would discourage the use of a naming strategy for the line drawings. Since people would not necessarily know if their verbal label would be sufficient to distinguish a target from a lure, more emphasis might be placed on the pictorial image. With greater control over item recognition and encoding strategies, it may be possible to determine which model—the quantitative or the qualitative—accounts best for the data.

Since cuing did not serve the purpose of exaggerating the hypothesized effects and since people in the postcued condition utilized a wide variety of second-guess strategies, cuing was dropped as a variable. All trials were precued. The one-to-one input condition was also dropped, as resolution of observed–predicted functions could not be obtained using the current design. Hence, only temporal or spatial reconstructions were utilized.

EXPERIMENT 2

Method

Design. The design included stimuli (words and line drawings) as a between-subjects factor and reconstruction task (temporal or spatial), halves, and serial position as within-subjects factors. The 12 subjects in each stimulus condition received four temporal and four spatial trials in alternation during each half of the session. All trials were precued.

Subjects. The subjects were 24 students in a memory class at the University of Toronto, who were fulfilling a course requirement. Subjects were assigned to the stimulus conditions in alternation at the time of their arrival at the laboratory.

Apparatus and stimuli. The apparatus was identical to that used in Experiment 1. The original stimuli were supplemented by 64 new words and

64 new line drawings, selected according to previous criteria.

Four sets of 8 test lists were drawn from the original stimulus ensemble and 4 sets of 8 lure lists were drawn from the new sets of stimuli. For each set, 8 reconstruction decks were created by combining a test list and a lure list. Appropriate forms of counterbalancing similar to those used in Experiment 1 were employed such that each subject essentially received a unique test set.

Each subject received 2 sets of 8 trials. In the first half, all items (test and lure) were new prior to presentation. In the second half, all items had been seen before but in different combinations. Throughout, test items came from the original stimulus ensemble and lures from the new ensemble. Thus, second half testing closely approximated Experiment 1, since by the second half, subjects could be quite certain which were *old* stimuli and which were the lures.

Procedure. The procedure was identical to that of Experiment 1, with the exception of the actual reproduction task. After presentation of the target items, the subject was handed a deck of 16 items and was instructed to go through the deck, card by card. As an *old* item appeared, it was to be placed in it's proper location. At any time during this process, the subject was free to rearrange previously placed items or to remove them. If the subject had not placed 8 items after going through the entire deck, he or she was instructed to pick out the most likely items and place them accordingly.

Results and Discussion

First, consider item recognition. In general, recognition levels were quite high. As a result, serial position was eliminated from the analysis (due to problems associated with unequal variances). The data base consisted simply of the total number of items correctly recognized per trial. For convenience, the results of the analysis will be discussed in terms of percentage recognition errors. Significance levels were set at .05. Fewer recognition errors were made on line drawings (4.5%) than on words (9.5%), $F(1, 22) = 4.42$, $MS_e = .53$. Not surprisingly, second half performance was superior to first half performance, $F(1, 22) = 8.76$, $MS_e = .20$. Two interactions were significant: Stimulus × Task, $F(1, 22) = 7.87$, $MS_e = .30$, Stimulus × Task × Halves, $F(1, 22) = 8.63$, $MS_e = .35$. Data pertinent to the triple interaction are shown in Table 2. The point of interest involves first half performance, when all items were new.

TABLE 2

Percentage Item Recognition Errors as a Function of Stimulus, Task, and Halves for Experiment 2

Condition	Reconstruction task: Temporal	Reconstruction task: Spatial
First half		
Line drawing	6.0	4.1
Word	7.5	15.1
Second half		
Line drawing	3.9	3.9
Word	7.3	7.8

Twice as many recognition errors were made on spatial trials than on temporal trials when the stimuli were words. For line drawings, the trend was for more recognition errors to occur on temporal trials. In the second half, when items had potentially been identified as *targets* or *lures* because of past experience, type of task had no differential effect on item recognition.

Two analyses were performed to assess memory for the temporal and spatial structures, both using the factorial design mentioned before. In the first analysis, the data base consisted of the number of correct placements (strict scoring) for each serial position, summed over the four replication trials per half. The second analysis compensated for item recognition effects by dividing the number of correct placements from the first analysis by the number of items correctly recognized. Thus, effects of differential item recognition levels could not be responsible for the obtained results. Data pertinent to both the absolute and the con-

TABLE 3

Temporal and Spatial Reconstruction Accuracy as a Function of Type of Stimulus in Experiment 2

Stimuli	Reconstruction task: Temporal	Reconstruction task: Spatial	*M*
Words	.57 (.61)	.39 (.42)	.48 (.52)
Lind drawings	.68 (.71)	.65 (.67)	.66 (.69)
Task *M*	.64 (.66)	.52 (.55)	

Note. The first number represents absolute proportion of correct placements, while the number in parentheses represents the conditional probability of a correct placement given the level of item recognition.

ditional analyses are shown in Table 3. All reported F values are reliable at the .05 level or beyond.

The results of both analyses were essentially identical. The use of line drawings as stimuli led to greater reproduction accuracy than did the use of words: $F(1, 22) = 15.48$ for the absolute analysis; $F(1, 22) = 14.89$ for the conditional analysis; $MS_e = 6.73$ and .39, respectively. Overall, temporal structures were remembered best: $F(1, 22) = 13.16$ and 16.03 for the absolute and conditional analyses, respectively. These effects were qualified by a Stimulus × Task interaction, $F(1, 22) = 6.68$ and 7.61 for the two analyses, respectively. The mean square errors for the above interactions for the two analyses were 2.76 and .14, respectively. For words, retention of spatial structures was very poor relative to retention of temporal structures, whereas for line drawings, both structures were equally well retained. The only other reliable effect was serial position: $F(7, 154) = 28.40$ for the absolute analysis; $F(7, 154) = 26.92$ for the conditional analysis; $MS_e = 1.57$ and .06, respectively. The primacy portion of the curve was higher than the recency portion and the asymptote was at Position 6.

The distributions of errors were also examined and are shown in Figure 3. The functions were similar to those obtained in Experiment 1, in that the temporal gradient was steeper than the spatial gradient, and this effect was most pronounced for words. The same pattern was found in both halves of Experiment 2.

Overall, the results from Experiment 2 were very simple. Even when, or especially when, recognition levels were controlled, a vast discrepancy between the retention of temporal and spatial structures arose for words but not for line drawings. Furthermore, when item recognition levels were assessed, it appeared as though the effort needed to process and retain spatial information overloaded the system sufficiently to interfere with word recognition. When temporal structures were to be retained, the effect on word recognition was much smaller. Thus, even the item recognition data support the proposed affinity between linguistic materials and temporal structures. The reverse trend, for example, more item recognition errors under temporal task requirements, occurred for line drawings. However, the error rates were so low that this evidence can only be viewed as sug-

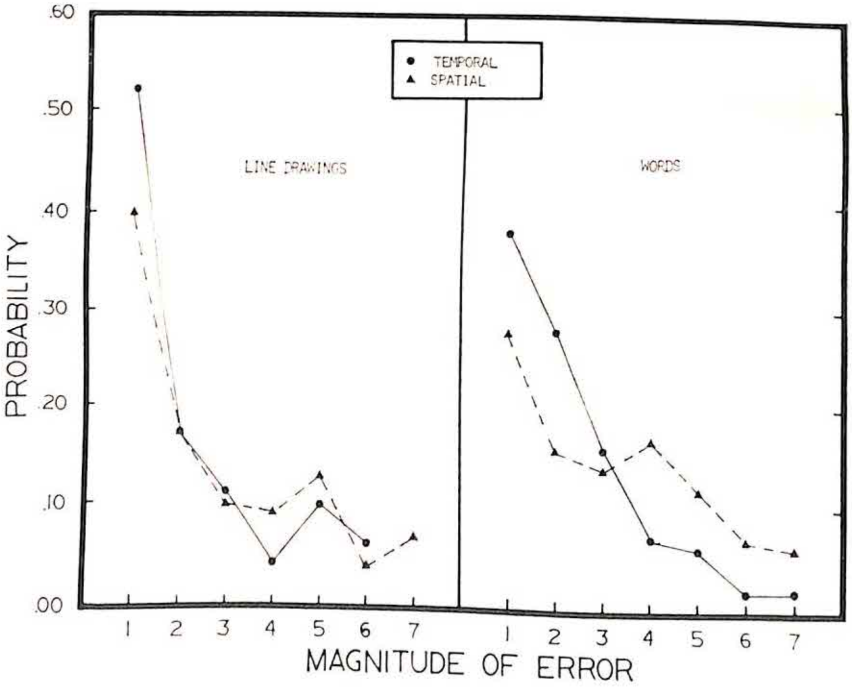

FIGURE 3. Probability distribution of the magnitude of erroneous placements in the temporal and spatial reconstruction tasks for the line drawings and words used in Experiment 2.

gestive of an affinity between pictorial materials and spatial structures.

Neither the quantitative nor the qualitative models were clearly supported. Only half of the predictions of the qualitative model were met (temporal superiority for words and different patterns for words and pictures). The quantitative model, on the other hand, received virtually no support, as similar trends across form classes did not emerge. Since the temporal/spatial disparity for linguistic materials has been well documented, the problem now is to determine whether it is possible to produce spatial superiority with pictorial materials or not.

Two factors, previously uncontrolled, may be responsible for the failure to produce spatial superiority for pictorial materials: (a) naming strategies and (b) base performance levels. Concerning the first, if verbal labels were applied to the line drawings, people conceivably may have both a verbal and an imaginal code to help them retain the two structures à la dual code theory. People in Experiment 2 did report using verbal strategies, especially in the second half of the session, in spite of the recognition procedure. Clearly, it is important to control labeling behavior.

Previously, it was hypothesized that trends within form classes could be studied independently of baseline performance levels. However, in Experiments 1 and 2 the observed temporal/spatial patterns have covaried perfectly with performance levels: At high performance levels, temporal/spatial equality has been observed, and at low levels, a disparity has been found. As noted before, low interitem similarity may be responsible for the picture superiority effects, and most likely, these line drawings are more dissimilar to one another than are the words. If this is the case, interitem similarity may be influencing not only item recognition but also ease of retention of the temporal/spatial structures.

Unfortunately, it is difficult to scale interitem similarity across form classes. However, it is relatively easy to frame the interitem similarity of words by using two sets of pictorial materials that are of low and high levels of interitem similarity. The line drawings used previously would be representative of a low interitem similarity set. Another set of stimuli that intuitively seem to have high interitem similarity and that are also difficult to label are line drawings of complex three-dimensional geometric figures.

In Experiment 3, all three types of stimuli (line drawings, geometric figures, and words) were used on a within-subjects basis. Unless the within variation produces complications, the trends noted for words and line drawings should be replicated. If geometric forms also give rise to temporal/spatial equality, it is likely that neither labeling behavior nor performance level (interitem similarity) is responsible for that equality. If either factor is important, however, spatial superiority may be observed in accordance with the dual code hypothesis.

An additional manipulation of the encoding variable was introduced by the use of instructional sets. Half of the subjects were instructed to attempt to encode all items pictorially, and the other half were instructed to use verbal strategies throughout. To the extent that imposed encoding strategies can influence temporal/spatial retention levels, the Stimulus × Task interactions should be exaggerated when *same* code instructions (for words, the code *verbal;* for pictures, the code *pictorial*) are given and minimized when *different* code instructions (for words, the code *pictorial;* for pictures, the code *verbal*) are given.

The recognition–reproduction task of Experiment 2 was again used. This time, lures and targets came from the same set, and lists were constructed by sampling with replacement. This procedure was used in an attempt to increase the number of recognition errors, so that the effect of reproduction task requirements on item recognition levels could be further assessed.

EXPERIMENT 3

Method

Design. The design included instructional set (image and verbal) as a between-subjects factor and stimuli (line drawings, geometric forms, and words), reconstruction task (temporal and spatial), and serial position as within-subjects factors. The 15 subjects in each instruction condition received

3 temporal and 3 spatial trials on each type of stimuli, in random order during one session.

Subjects. The 30 subjects were University of Toronto summer school students in a course on human memory. Participation in the experiment fulfilled part of their course requirements. Subjects were assigned to the instructional set conditions in alternation upon their arrival at the laboratory.

Apparatus and materials. The apparatus was the same as used in Experiments 1 and 2. The original set of 64 line drawings and 64 words were modified such that no word named or was directly related to any of the line drawings. An additional set of 64 line drawings of complex three-dimensional geometric figures, similar to those found in the Space Relations test (Bennett, Seashore, & Wesman, 1963), were developed.

Two basic presentation orders were developed as follows. For each type of stimuli, 7 sets of 16 items each were selected by sampling with replacement up to two draws. Eight of the 16 items in each set were randomly designated as target items and 8 as lures. Any item repeated across lists could occur either twice as a target, twice as a lure, or once as a target and once as a lure. The testing sequence of the 21 trials was ordered such that 1 trial on each stimulus type occurred in each block of 3 trials. Within each block, the order of stimulus type was random. Temporal and spatial tasks were randomly assigned to the trials with the restriction that each stimulus type be tested on both tasks within each block of 6 trials. For each of the presentation orders, 4 of the subjects in each instructional set received the trial sequence in the forward order and 3 received the backward order. Half of the subjects in each of these subsets received the original temporal/spatial assignments; for the remaining subjects, the task assignments were reversed. The first 3 trials of the testing sequence were designated as practice trials and were never scored.

Procedure. With the exception of encoding instructions, the procedure was identical to that used in Experiment 2. At the beginning, subjects were instructed to use one encoding strategy to the best of their ability. Subjects receiving image instructions were told to try to attend to and retain the pictorial aspects of the pictorial materials; for words, they were to concentrate on generating a mental image of the object named by that word. Subjects receiving verbal instructions were asked to attend to the linguistic character of the words, to attend to the name of the line drawings, and to attempt to name or verbally describe the geometric forms.

TABLE 4

Temporal and Spatial Reconstruction Accuracy as a Function of Type of Stimulus in Experiment 3

Stimuli	Reconstruction task		*M*
	Temporal	Spatial	
Words	.60 (.66)	.45 (.51)	.53 (.59)
Line drawings	.68 (.70)	.66 (.70)	.67 (.70)
Geometric forms	.48 (.54)	.47 (.55)	.47 (.55)
Task *M*	.58 (.63)	.53 (.59)	

Note. The first number represents the absolute proportion of correct placements, while the number in parentheses represents the conditional probability of a correct placement given the level of item recognition.

While subjects were precued as to the type of task, they did not know which type of stimulus they would see until the first slide of each trial appeared. Subjects were forewarned that items could be repeated and that the repetition could be either a target or a lure.

Results and Discussion

Throughout, all reported F values exceeded the .05 level of significance. As before, item recognition was quite high and serial position was not included as a factor in the analysis. For convenience, the data are presented in terms of percentage error, though the analysis was performed on the total number correct per trial. Overall, fewest errors were made on line drawings (4.5%), an intermediate number on words (11.7%), and most on geometric figures (15.3%), $F(2,\ 56) = 34.15$, $MS_e = 1.01$. More errors were made on spatial trials (11.7%) than on temporal trials (9.3%), $F(1,\ 28) = 5.76$, $MS_e = .84$. Instructional set had no effect on recognition levels nor was there any indication of a Stimulus × Task interaction. Apparently, the effect of task requirements on item recognition found in Experiment 2 was unreliable.

As in Experiment 2, the reconstruction protocols were analyzed in terms of (a) the absolute number of correct placements (strict scoring) and (b) the probability of a correct placement given the level of item recognition. In both analyses, the data were collapsed across the three replication trials per condition for each serial position. Neither instructional set nor reconstruction task was significant as a main effect in either analysis. Stimulus type did affect reconstruction performance: $F(2,\ 56) = 34.78$ for the absolute analysis; $F(2,\ 56) = 20.69$ for the conditional analysis; $MS_e = 1.30$ and .14, respectively. As can be seen in Table 4, performance was best for line drawings and poorest for geometric forms.

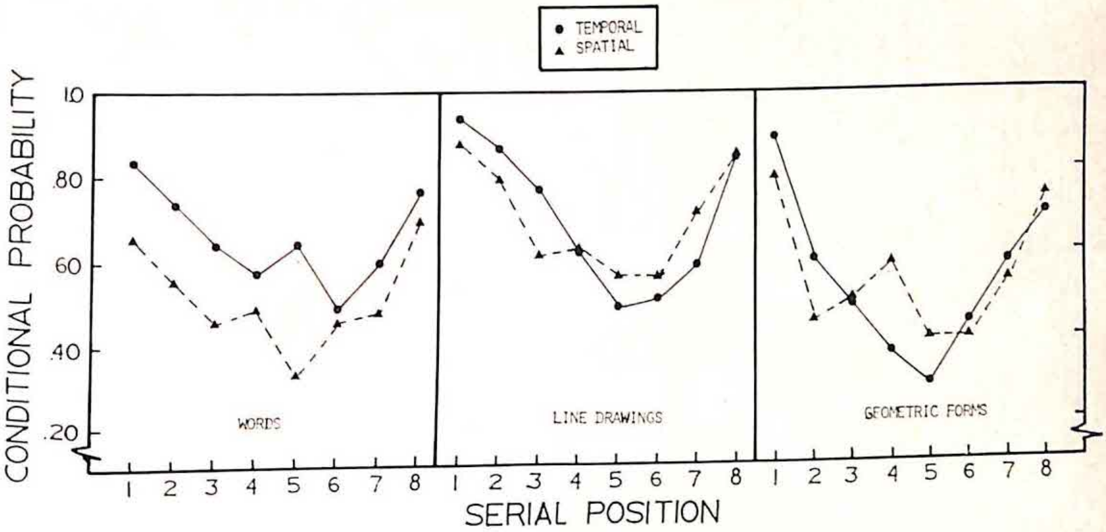

FIGURE 4. Probability of correct temporal and spatial placements given item recognition as a function of serial position for the words, line drawings, and geometric forms used in Experiment 3.

Both analyses also revealed a significant Stimulus × Task interaction: $F(2,\ 56) = 5.46$ and 6.15, $MS_e = 1.19$ and $.15$, for the absolute and conditional analyses, respectively. As shown in Table 4, temporal and spatial reconstruction levels were essentially equivalent for both types of pictorial materials. However, temporal superiority emerged for words. Since geometric figures resulted in low performance levels and were difficult to label, the temporal/spatial equality observed with pictorial materials cannot be attributed to effects due to naming strategies or performance levels (interitem similarity).

Serial position was significant in both analyses and was qualified by several interactions. The Task × Serial Position interaction was reliable in both the absolute and conditional analyses, respectively: $F(7, 196) = 5.31$ and 2.23; $MS_e = .58$ and $.08$. In general, the primacy effect was smaller for the spatial task, while the recency effect was equal for the two tasks. In the absolute analysis, stimulus also interacted with serial position, $F(14,\ 392) = 2.25$, $MS_e = .61$. Both types of pictorial materials produced more bowed serial position curves than did the words. A similar, though unreliable, trend was noted in the conditional analysis, $F(14,\ 392) = 1.61$, $p < .10$, $MS_e = .08$.

The Stimulus × Task × Serial Position interaction was significant in both analyses: $F(14, 392) = 2.18$, $MS_e = .48$ for the absolute analysis; $F(14,\ 392) = 1.91$, $MS_e = .07$ for the conditional analysis. The interaction, using the conditional data, is shown in Figure 4. For all types of stimuli, temporal superiority was observed in the primacy portions of the curve. This effect was largest and extended further into the curve for words and was smallest and shortest for geometric forms. This pattern of temporal superiority in the primacy portion of the curve may reflect the relative ease of temporal rehearsal and the impact of nameability on rehearsal. In the recency portion of the curve, temporal superiority was reduced for words, while for line drawings, spatial superiority emerged. For geometric forms, temporal/spatial equivalence was obtained in the recency portion, though the spatial asymptote was higher than the temporal asymptote. These effects may reflect the immediate compatibility of spatial structures and pictorial processing. That is, while spatial structures may be more difficult to rehearse and maintain, they may register well initially, especially when pictorial stimuli are being processed.

Finally, instructional set interacted with stimuli and serial position in the conditional

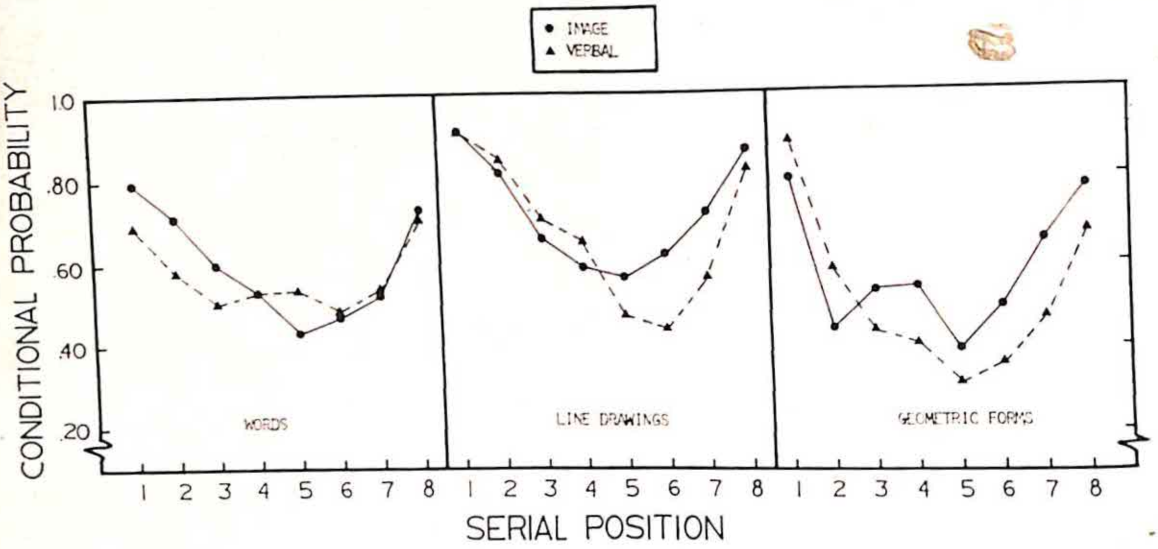

FIGURE 5. Probability of correct placements given item recognition under image and verbal encoding instructions as a function of serial position for the words, line drawings, and geometric forms used in Experiment 3.

analysis, $F(14, 392) = 2.10$, $MS_e = .08$. Though not reliable, a similar trend was noted in the absolute analysis, $F(14, 392) = 1.62$, $p < .10$, $MS_e = .61$. The interaction is shown in Figure 5. For words and geometric figures, instructions that led to dual encoding (*image* for words and *verbal* for geometric forms) elevated the primacy portion of the curve relative to the single encoding conditions. For line drawings, the effect of dual versus single encoding was minimal, as though dual encoding occurred in spite of instructions. Such results are in keeping with the notion that a dual code results in a more durable memory trace than a single code. In addition, dual code conditions led to a sizable decrement in the recency portion of the curve for both types of pictorial materials relative to single code conditions. Over the short run, attempts to use a dual code may result in poorer initial registration, though what is registered may be more durable in the long run. If it can be argued that dual code conditions resulted in more elaborative or deeper codes than did single code conditions, the results found here seem to parallel the findings of Bjork and Jongeward as reported by Bjork (1975). They manipulated type of rehearsal of word sextets and found that at short retention intervals, primary (rote) rehearsal frequently led to higher retention levels than did secondary (elaborative) rehearsal. At longer retention intervals, the reverse was true.

In general, the effects of imposed encoding strategies were minimal, contrary to expectations. It is unlikely that subject cooperation was a factor, as most subjects reported 75%–100% success in applying the strategies. Though not conclusive, it would appear that encoding strategies applied above and beyond those activated by the stimuli themselves only serve to modify portions of the codes produced by the intrinsic encoding.

As before, error distribution analyses were performed and are shown in Figure 6. Steep error gradients were found for both temporal and spatial tasks. For words, the spatial curve was somewhat more shallow than that observed for the temporal task. For pictorial materials, the curves were almost identical. No distinct differences were noted as a function of type of encoding strategies.

GENERAL DISCUSSION

The retention of temporal and spatial structures was greatly affected by the form class of stimuli used in the test. Pictorial forms gave rise to equality of temporal and spatial retention, regardless of base performance levels. Use of linguistic forms led to consistent temporal superiority. These effects held across both within- and between-subjects designs. The use of encoding strategies extrinsic to the form class in question minimally affected temporal/spatial retention levels and/or patterns, whereas the processes evoked by the stimuli themselves greatly influenced the temporal/spatial patterns. That is, the processes used in the perception and encoding of pictorial materials appear to be equally compatible with the processing and retention of both types of structures, whereas processes used to perceive and encode linguistic materials appear to be most compatible with the processing and retention of temporal structures, as determined by performance in this paradigm.

Only qualitative models received support. According to the quantitative model, similar trends should have been observed across the form classes, and this certainly did not occur. The dual code hypothesis predicted temporal superiority with linguistic materials and spatial superiority with pictorial materials. Predictions from this qualitative model were upheld in that different temporal/spatial patterns were found for the two form classes, and linguistic material did result in temporal superiority. However, spatial superiority was not found with pictorial materials. Unless a valid explanation can be found for the failure to produce the expected spatial superiority, the dual code hypothesis must be revised. Before attempting such revision, it is appropriate to consider several issues that may have bearing on the observed temporal/spatial equality obtained with pictorial materials.

It might be argued that the spatial task was impoverished in that only a linear array was used. Surely, spatial information must encompass more than one dimension. However, as time is unidimensional, the only valid spatial comparison must also be unidimensional. These experiments certainly do not give a complete picture of the relationship between the two form classes and the processing and retention of extended spatial information. While investigation of this relationship may be illuminating, it is out of bounds for the question at hand.

One might then suggest that the spatial separation was not as discriminable as the

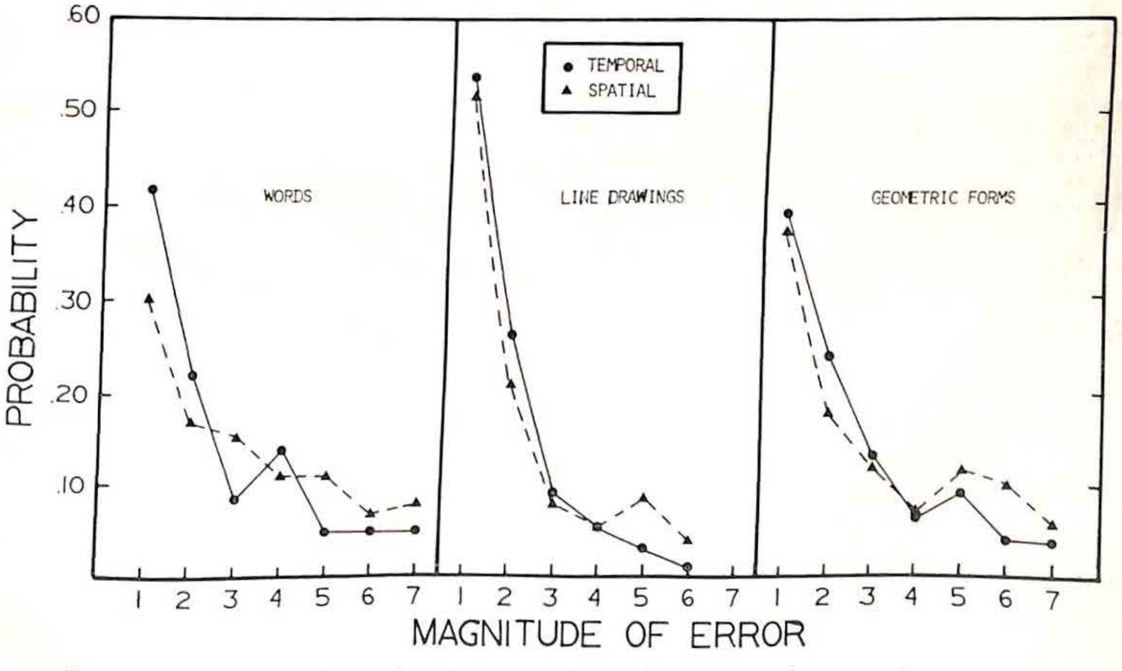

FIGURE 6. Probability distribution of the magnitude of erroneous placements in the temporal and spatial reconstruction tasks for the words, line drawings, and geometric forms used in Experiment 3.

temporal separation used in the present experiments. However, quite different temporal and spatial separations were used in the Mandler and Anderson (1971) experiments, yet the magnitude of the temporal superiority observed with words in their experiments was comparable to that observed in the present experiments. More importantly, the fact is that pictorial and linguistic forms gave rise to different temporal/spatial patterns at constant temporal and spatial settings.

One might finally suggest that the failure to find spatial superiority was due to the apparent differential rehearsibility of temporal and spatial structures. Cuing drastically affected temporal retention in Experiment 1, though it barely influenced spatial retention. In Experiment 3, temporal primacy was always greater than spatial primacy and especially so for linguistic materials. Both effects could be the result of greater rehearsibility of temporal structures. Thus, even though spatial structures may be processed more efficiently when pictorial materials are used, the ease of temporal rehearsal may compensate for the initial difficulty in processing the temporal information when pictorial materials are used.

The notion of differential rehearsibility raises some intriguing questions about the nature of temporal and spatial structures. Intuitively, it seems that in these experiments, the temporal structure was given by the order of presentation. Spatial structures, on the other hand, were not given directly by presentation order but had to be constructed. Hence, apparent differential rehearsibility of the two structures may actually be the result of differences in the ease of constructing the structure. Healy (1975a) has argued that paradigms of the type used in the present experiments cannot be used to answer questions concerning temporal or spatial superiority because of the aforementioned asymmetry in the ease of constructing temporal and spatial structures. She claims that this paradigm reduces to a paired-associate task, where the pair members are the to-be-remembered items and their serial positions (temporal or spatial, depending upon the recall task). Except when temporal and spatial structures are in one-to-one correspondence, it is always true that both items and their serial positions are presented in a constant order in temporal recall tasks, whereas neither items nor their serial positions are presented in a constant order in the spatial recall task. Healy eliminated this asymmetry by presenting to-be-remembered items in a constant temporal order for the spatial recall groups and in a constant spatial order for temporal recall groups. The to-be-remembered items were four consonants that remained constant over a series of 72 trials. Under these testing conditions, temporal and spatial structures were recalled equally well at short retention intervals, leading Healy to conclude that neither structure was more or less difficult to encode than the other, once the basic asymmetry had been corrected. Mandler and Anderson (1971) also performed a variant of this manipulation. In their first two experiments, people in different conditions received either two or four study trials, where the 12 to-be-remembered words were presented in constant temporal and constant spatial orders. They found consistent temporal superiority. Temporal recall was 94% after two study trials in which type of recall was precued, and 92% after four study trials in which recall mode was postcued; spatial recall in the two conditions was 72% and 64%, respectively. These results are at variance with Healy's findings. While Mandler and Anderson's and Healy's experiments differed on many dimensions (e.g., type of stimuli, amount of practice, and list length), the critical variable may be list length, as will be explained below.

Temporal and spatial structures may be constructed from either absolute or relative positional information. Healy's analysis (1975a) considers only absolute information. If both types of positional information are taken into account, a paired-associate analysis of the types of tasks used in the present experiment and Healy's experiments must include two sets of associations: those between the to-be-remembered item and its absolute serial position (temporal or spatial) and the associations between the item and its position relative to items temporally or spatially adjacent to it. For temporal recall tasks, both absolute and relative associations are always given by the order of presentation in a constant order. For spatial recall tasks, only information involving absolute serial position can be given in a constant order, as in Healy's experiments. Information concerning relative spatial position can never be given directly in these paradigms; those associations must be constructed.

The basic question is, Can people use absolute position information alone, and if so, under what conditions? When the number of positions to be retained is less than the span of apprehension, as in Healy's (1975a) experiments, the information provided by absolute serial position may be sufficient to encode both temporal and spatial structures efficiently. This, of course, only applies when the asymmetry between temporal and spatial structures regarding absolute position information has been eliminated (cf. Healy, 1975b). However, when the number of positions to be remembered exceeds the limits of the span of apprehension, as in the Mandler and Anderson (1971) experiments, absolute time and space cues may be no longer sufficient to encode the structure. Since relative spatial position must be constructed, temporal superiority may well emerge when people have to rely on relative as well as absolute positional information.

These notions clearly need experimental verification. However, if they are correct, spatial structures may be more difficult to construct (and rehearse) than temporal structures when information is presented discretely in time and space. Before attempting to interpret the patterns of temporal and spatial reconstructions found in the present experiments, it may be useful to compare the way temporal and spatial information was presented in the present paradigm with the realities of day-to-day presentation of similar types of information. It will be argued that, in many respects, the paradigm used does reflect the way temporal and spatial information typically enters the processing system.

Temporal information, by definition, arrives sequentially in time. Often, the spatial location of the items in a temporal sequence is not displaced in space, though it may be. However, spatial displacement does not seem to affect the presentation of the cues said to be used in the construction of a temporal structure. That is, whether the items are displaced in space or not, both absolute and relative temporal position information are provided in a constant order and are given by the order of presentation.

Spatial information, at least from displays larger than those that can be processed by a single fixation, is picked up by successive glances in time, where the temporal sequencing of these glances need not correspond to the structure being perceived. This may seem counterintuitive, for the elements of a spatial array are typically presented simultaneously, and we have a sense of simultaneity of perception. However, visual acuity decreases rapidly as information is presented further into the peripheral regions of the eye, such that beyond 5° of visual angle from the center of a fixation, even word shape identification is very poor (Rayner, 1975). To accurately encode an item foveal processing is necessary; hence, in viewing a large display several fixations, usually on areas high in information value, are needed to encode the entire array. During each fixation, details of the fixated object are extracted via foveal processing, while peripheral information is analyzed to determine where to look next (Loftus, 1976). Peripheral information must contribute to the development of a spatial structure, but the extent of its contribution in large displays is unknown. Close to foveal regions, the contribution may be extensive; further out, it can contribute very little, for at the far reaches of the periphery, only movement and sudden changes in illumination can be detected.

The argument advanced here, then, is that even in arrays where all elements are presented simultaneously, much of the spatial information will arrive sequentially in time. To construct a mental representation of the spatial relations, information obtained from the present fixation must be integrated with memory for the positions of previously viewed items. Unless the fixations have been made in a series of left-to-right (or vice versa) steps, neither absolute nor relative position information will be given by the temporal sequencing of fixations. And, in that sense, the presentation of spatial information over time, as in the present experiments, does reflect the way spatial information enters the processing system.

One difference between normal spatial processing and spatial processing as it can occur in this paradigm is that, in the former case, a person can consult the actual display to verify the correctness of his or her mental representation, whereas in the latter situation, no external verification is possible. This may explain why people found it difficult to rehearse the spatial structures. Under normal viewing conditions, the spatial positions are available for multiple looks, thereby reducing the need to develop efficient mental rehearsal routines. A second difference is that peripheral visual information could not be utilized in this paradigm, whereas it can be used under normal viewing conditions. As noted above, the importance of peripheral information in the construction of spatial structures has yet to be determined. Both of these differences arose because of the need to equate temporal and spatial presentation conditions such that they could be compared legitimately. That is, since it is impossible to reexperience a temporal sequence except via memory, it was necessary to remove the possibility of multiple examinations of the same spatial position in the spatial task. Investigation of the effects of successive versus simultaneous presentation of spatial information in large displays is clearly needed, and such investigations may reveal much about the nature of spatial perception and encoding.

The general predictions of the dual code hypothesis were clearly supported, since the temporal/spatial patterns did differ as a function of form class. The end result of greater temporal accuracy for linguistic materials and equal retention of temporal and spatial structures with pictorial materials seems to correspond with day-to-day processing demands. When it is necessary to localize auditory linguistic signals, such localization tends to depend on the use of many acoustic and visual cues in conjunction. For written materials, the location of the printed word is quite irrelevant as long as it follows the preceding word in the standard left-to-right fashion. That is, we do not need to remember whether a particular word occurred near the top or the bottom of the page, or to the right or left of center. In both visual and auditory cases, the ability to perform efficient sequential processing on linguistic materials is mandatory. In the nonlinguistic world, both temporal and spatial information become important at various times. In constructing a mental representation of a room, the temporal order of input is relatively unimportant. However, when attempting to manipulate a crowded freeway, the necessity for efficient temporal and spatial information processing is paramount.

Because spatial structures may be more difficult to process than temporal structures, it is difficult to determine the exact relationship between temporal and spatial processing efficiency as a function of form class. That is, the observed temporal superiority for linguistic materials may reflect the fact that temporal processing is facilitated relative to spatial processing when applied to linguistic materials. Alternatively, temporal superiority may simply reflect differential task difficulty. On the other hand, if differential task difficulty does exist, the pattern of temporal/spatial equality observed with pictorial materials must reflect facilitation of spatial processing relative to temporal processing efficiency when these routines are applied to pictorial forms. Future research investigating the nature of temporal and spatial structures is needed to determine how these structures are processed and encoded and to determine whether there is an inherent asymmetry in the ease of constructing these structures. Until these questions can be answered, the specific relationship between the processing of temporal and spatial structures and form class cannot be determined.

The nature of the imaginal and verbal codes, as postulated by the dual code hypothesis, appears to need revision. The original statements seemed to imply that the codes aroused by encoding strategies extrinsic to the stimuli were very similar in quality (though maybe weaker) to the codes elicited by the stimuli themselves. This appears to be false. The fact that imposed encoding strategies had little effect on the general trends (see Experiment 3) seems to imply that extrinsic imaginal and/or verbal encoding strategies do not fully mimic the encoding processes elicited by the stimuli. That is, while it is true that encoding strategies can dramatically affect item recognition (Paivio, 1971), the same strategies do not seem to affect temporal/spatial retention. Since Paivio (1971) has reported that the arousal of an image takes .6 to 1.0 sec, it would seem that the total presentation time of 2.2 sec/item should have been sufficient to produce an effect. Since it did not, the relationship between imposed and intrinsic coding processes must be examined further for the dual code hypothesis to become a useful theory.

Finally, a note is in order about the proposal that the imaginal code results in a more durable memory trace than does the verbal code. Either pictorial or verbal superiority may be produced by careful stimulus selection (see item recognition, Experiment 3). Until words and pictures can be equated on meaningful psychological dimensions, the superiority of one or another code simply cannot be determined. Perhaps the most effective research strategy would be to search for qualitative effects first. Once processing differences and similarities are better understood, quantitative effects may become more tractable. At that stage, it may also be possible to equate pictures and words so that a direct comparison of processing difficulty can be attempted.

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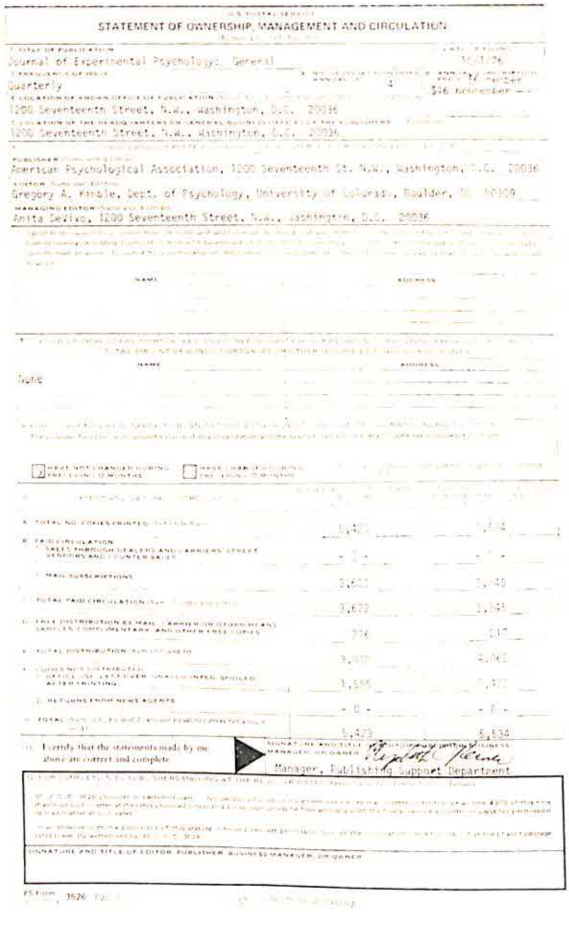

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Model-Based Decision and Inference in Stud Poker

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University of Wisconsin—Madison

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SUMMARY

Three experiments study how a poker player's models or mental representations of the game influence his play in a modified version of five-card stud. Specifically, the experiments focus on (a) the evaluation of the likelihood that one can beat a particular opponent and (b) the combination of two or more such likelihoods into a global judgment of the likelihood of winning the hand.

In Experiment 1, experienced poker players were presented with pairs of opposing poker hands, each of which were described by the upcards in the hand, the amount bet on the hand by the opponent, and the playing style of the opponent. They judged the likelihood that a standard pair of 7s could beat the opposing hands: First, they rated the likelihood directly, and second, they chose an amount to wager with the experimenter in a real side-bet on the outcome of the hand. The results for both conditions indicated that the subjective likelihood of beating a pair of poker hands is a multiplicative function of the subjective likelihoods of beating each of the hands individually. Furthermore, the results of the betting condition suggested that subjects bet proportional to their subjective likelihood of winning.

In Experiment 2, experienced poker players made side-bets against single hands described by upcards, amount of opposing bet, and playing style of opposing player. The most striking aspect of the data was a nonmonotonic relationship between the amount of the bet and the objective threat of the upcards. Two possible explanations for this result are discussed: (a) a simple bias in the subject's model of the mathematics of the game and (b) an active strategic discounting of hands on which the opponent is likely to be bluffing.

Experiment 3 had four parts. In Part 1, the test of the multiplicative combination rule for pairs of opposing hands was replicated and confirmed at the level of the single subject. Part 2 tested the mathematical bias hypothesis for the nonmonotonicities in Experiment 2. Two of seven subjects showed the mathematical bias. Part 3 tested the bluff-discounting hypothesis for the nonmonotonicities. Bluff-discounting was clearly demonstrated for another two of the seven subjects. Part 4 examined the rule for combination of upcard information and bet-amount information into a subjective likelihood of beating the hand. The results support an averaging rule for two subjects and an adding rule for five others.

Experimental studies of decision making have concentrated on tasks for which the optimal response is mathematically well defined. The predominant research strategy has been to assume a prescriptive model drawn from mathematical decision theory, and then to compare human performance with optimal performance. Often the conclusions drawn from these studies have not been optimistic about man's capabilities as a decision maker. Not only is he nonoptimal in his decision making, but he appears to be bound by the literal aspects of the stimulus, neither transforming incoming information according to need, nor bringing new information into the decision situation. (For a review of the limitations of man's decision-making capabilities, see Slovic, 1972a.)

These conclusions may be hasty, at least insofar as they are generalized to behavior in other task environments. To be specific, decision making has been studied in environments for which the statistically sophisticated researchers have appropriate models, whereas the statistically naive subjects have none. While it is undoubtedly true that people perform poorly under these circumstances, it does not follow that their performance will be poor in familiar environments for which they have developed appropriate models. This article presents research on decision processes in an environment for which the subjects do have appropriate models. In particular, the article examines inference and decision processes among experienced players of stud poker.

Stud Poker

Stud poker is not a game of chance: It is a game of partial information. Intelligent use of that information requires that a player develop models or mental representations of both the mathematical structure of the game and the psychological structure of the opponent. The models of mathematical structure provide rules for making inferences about how the cards fall: They include the player's beliefs about factors like randomness, independence, and the likelihood of joint events. The models of psychological structure provide rules for interpreting the behavior of opponents: They include the player's beliefs about betting pattern, playing style, when and how opponents bluff, and even what sort of image he projects himself.

In standard five-card stud, each player is dealt two cards: one, the *hole* card, face down, and one face up. A betting round follows in which players evaluate their own cards in light of opponents' bets and cards, and then decide whether to bet or fold. The players who bet remain in the game and are dealt a third card face up. Another betting round follows and players revalue their cards and their opponents' bets and cards, and again decide whether to bet or fold. This procedure continues until four cards have been dealt face up, and four rounds of betting have been completed. If only one player remains in the game, he takes the pot and need not show his hole card. If two or more players remain, there is a showdown with each player turning up his hole card to determine who has the winning hand.

The present research is based on a modified, nonsequential version of five-card stud in which each player receives all his cards, one down and four up, before any betting takes place. This game is simpler than standard stud but retains much of its character. In fact, it even has a few advantages; for example, more good hands

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can occur, since players who get off to a bad start with their first two cards do not drop out of the hand.

Information Integration Theory and Functional Measurement

The research presented here rests both theoretically and methodologically on Anderson's (1974a, 1974b) theory of information integration. This is a general theory of judgment that has been applied successfully to several problems in decision making, including risky decision making (Anderson & Shanteau, 1970; Shanteau, 1974), sequential decision making (Shanteau, 1970), and Bayesian inference (Leon & Anderson, 1974).

According to information integration theory, judgments of composite stimuli require two basic operations: First, the person must evaluate the meaning and relevance of each stimulus item on the dimension of judgment, and second, the person must integrate these component evaluations into a single response. The theory relies heavily on simple algebraic models of the integration operation expressed in subjective metrics. These, combined with factorial stimulus designs, allow simple and powerful tests of goodness of fit using ordinary analysis of variance. Furthermore, the allied technique of functional measurement (Anderson, 1970) allows the user to deal simultaneously with substantive theory and measurement: A single test of fit can jointly validate both integration model and response scale, and thereby permit interval scale measurement of the subjective values of stimulus items.

In the present article, information integration theory and functional measurement are applied to two fundamental processes in poker playing: first, the evaluation by a player of the strength of his various opponents' hands, and second, the integration of these individual evaluations into a global likelihood of winning the pot. To illustrate these processes, consider three situations that might confront Player *A* below:

7 8 J A ? Player *B*		3 4 4 A ? Player *C*
	9 10 10 Q ? Player *A*	

(Note that card ranks 2 through 10 are coded by arabic numerals; jack, queen, king, and ace are coded by J, Q, K, and A, respectively; and the face down hole card is coded by "?".)

1. If Player *A* has a *ten* in the hole, he knows that neither Players *B* nor *C* can beat him no matter what their hole cards are. Evaluation and integration are thus trivial, and Player *A* has only the strategic problem of choosing a bet amount that will encourage Players *B* and *C* to put their money in the pot.

2. If Player *A* has a *nine* in the hole, he can still ignore Player *B*, since *B* cannot possibly beat two-pair, but he cannot ignore Player *C*, since *C* could beat him with either three 4s or two-pair, aces-up. Player *A*'s problem then is to evaluate the likelihood that Player *C* can beat him. Two types of information are available to Player *A*. First, he can use his model of the mathematical game structure to interpret the objective threat of the opposing cards. Observing 12 upcards on the board and his own hole card, he may calculate either exactly or to a rough approximation that only four of the remaining 39 cards can make Player *C* the winner. Second, Player *A* can improve on this objective estimate by using his model of the opponent to interpret information from the betting. For example, suppose Player *A* checks, postponing his bet, Player *B* checks, and Player *C* opens with a moderate bet. Player *C* knows that Player *A* has at least a pair of tens, and *A* knows that *C* knows. Player *A* must interpret *C*'s bet in light of what he knows about poker players in general and Player *C* in particular. A risky Player *C* might be trying to bluff Player *A* into folding, so *A* might take *C*'s bet with a grain of salt. On the other hand, a conservative Player *C* is likely to have at least two-pair, so Player *A* must estimate the likelihood

that C holds the safe fours-up rather than aces-up or better.

3. If Player A has a *two* in the hole, either Player B or C or both might beat him. Thus, he must evaluate both the likelihood of beating Player B and the likelihood of beating Player C, and then integrate these individual likelihoods to get a global likelihood of winning the hand. Mathematically, there is a simple procedure for this: The probability of the joint event (Beat B and Beat C) is approximately equal to the product of the individual probabilities of beating B and beating C. Psychologically, however, multiplying is only one of the combination rules that Player A might use. For example, another possibility would be a disjunctive rule under which Player A would attend only to the more threatening hand.

Related Studies of Poker Playing

Experimental psychology. In the early 1920's, Riddle (1925) studied bluffing, risking, and a reaction tendency she called "desire to beat" during actual stud poker games. Her data included physiological reactions like heart rate and breathing, introspective reports of emotions directed at opponents, and numerical descriptions of game factors like rank of a player's hand and amount of money in his pile. Unfortunately, Riddle was hampered by the limited statistical tools of her time. Although she analyzed her data as best she could using correlation, the results are unwieldy and shed little light on decision processes in poker.

Economics. In their treatise on game theory, Von Neumann and Morgenstern (1944) derived optimal strategies of play for a simplified version of straight poker,[1] the earliest form of the game. In their game, each of two players is dealt five cards, which only the player sees and which cannot be replaced. Then the players bet simultaneously either low or high as to who has the best hand. If the bets are not equal, the player who bet low is given the option of forfeiting his bet (folding) or increasing it to high (calling).

Although Von Neumann and Morgenstern's game is greatly simplified, it does retain certain basic qualities of poker. For example, bluffing (or betting high even though one's hand is poor) is an element of the optimal game strategy. However, the authors found it difficult to relax even the simplest restrictions of their game, and they did not allow the interesting forms of partial information (e.g., cards drawn in draw poker or upcards in stud poker) that characterize all modern poker variations.

Artificial intelligence. Waterman (1970) used draw poker to test the proficiency of a game-learning computer program. Although Waterman's primary interest was in the production system that evaluates, modifies, and creates its own learning heuristics, his treatment of poker is interesting because it is based on the sorts of heuristics that human players might use. For example, the bluffingness of an opponent is estimated by simply counting the number of times he bluffs.

Findler and his associates (Findler, in press; Findler, Klein, Gould, Kowal, & Menig, 1972) use draw poker in their studies of decision making under uncertainty and risk. Their goals include the development of techniques for decision making based on imperfect information and the implementation of powerful automated learning routines. Among the many poker-playing programs they have written are several that base decisions on interesting combinations of machine-oriented mathematical rules and human-oriented heuristics.

Related Studies of Risky Decision Making

In poker each round of betting confronts the player with a risky decision. Such decisions have been intensively studied from the viewpoint of utility theory. Under utility theory, a rational decision maker chooses among the options open to him in such a way as to maximize utility or subjective value. In riskless choices he has only to evaluate the options and choose the one which has the greatest overall utility. In risky or uncertain choices, however, he has to weigh the options by their respective likelihoods, either actual or subjective, and choose the option for which the product of utility and likelihood is greatest.

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[1] Von Neumann and Morgenstern call the game stud poker. However, the game they describe bears no relationship to the modern stud poker game.

Following Von Neumann and Morgenstern (1944), many researchers took utility theory as a model of human decision making. However, it soon became apparent that experimental subjects systematically violated the theory's axioms and theorems. For example, subjective probabilities and outcomes of events were shown to interact (Irwin, 1953; Slovic & Lichtenstein, 1968b), sequence effects such as gamblers' fallacy were demonstrated in gamble preferences (Myers & Fort, 1963), intransitivities were shown for both risky and riskless choices (Tversky, 1969), and bet preferences were shown to be influenced by response mode (Lichtenstein & Slovic, 1973).

These results prompted attempts to shore up the rational model by adding psychological variables to the theory, including preferences for particular probabilities (Edwards, 1954), variances (Coombs & Pruitt, 1960), and levels of risk (Coombs & Huang, 1970; Pruitt, 1962). In the long run, however, these have not been sufficient to save the theory. Probability and variance preferences have been shown to be artifacts of decision strategy (Slovic & Lichtenstein, 1968a), risk preferences have been shown not to generalize across various response measures (Slovic, 1972b), and positive examples have been provided of decision making heuristics that are based on principles other than utility maximization (Anderson & Shanteau, 1970; Lichtenstein & Slovic, 1973; Shanteau, 1974). The final result has been that in recent years reviewers who are otherwise sympathetic to the goals and methods of utility theory have stated that the net contribution of the theory has been disappointingly slight (Lee, 1971, p. 104), and that the theory is applicable only to a narrow and unacceptable class of experimental tasks (Rapoport & Wallsten, 1972, p. 141).

The failure of utility theory to contribute substantially to the understanding of human decision processes may reflect its reliance on a prescriptive model. While such models are useful in applied contexts, they are almost certain to fail as psychological models. In contrast, the descriptive models of integration theory are more flexible and allow full scope for dealing with a diversity of possible decision-making mechanisms.

Experimental Task

It is difficult to analyze poker by observing the bets a player makes during a game. This is true for any form of poker, even the modified version. The cause of the difficulty is basic to the game itself: Good poker strategy requires a player's bets sometimes to vary directly with his subjective likelihood of winning, and sometimes, as in bluffing (betting high with a poor hand), and sandbagging (betting low with a good hand), to vary inversely. For this reason, the experiments reported here all involved an experimental task that, while based on the modified game, was not the game itself. The subjects were shown descriptions of opposing poker hands and were asked to judge how likely it was that these hands could be beaten by a standard hand, always a pair of 7s.

In all experiments, one dependent variable was the amount the subjects chose to risk in a real side-bet with the experimenter on the outcome of the hand. In addition, one experiment had the subjects rate their subjective likelihoods of winning directly. Note that these responses required the subject only to evaluate and to integrate the stimulus material. Strategy was not required directly, but only insofar as the evaluation process is based on a model of the strategies used by different types of opponents.

Stimulus hands. The opposing hands were each described by several pieces of information. Although the type of information varied somewhat from experiment

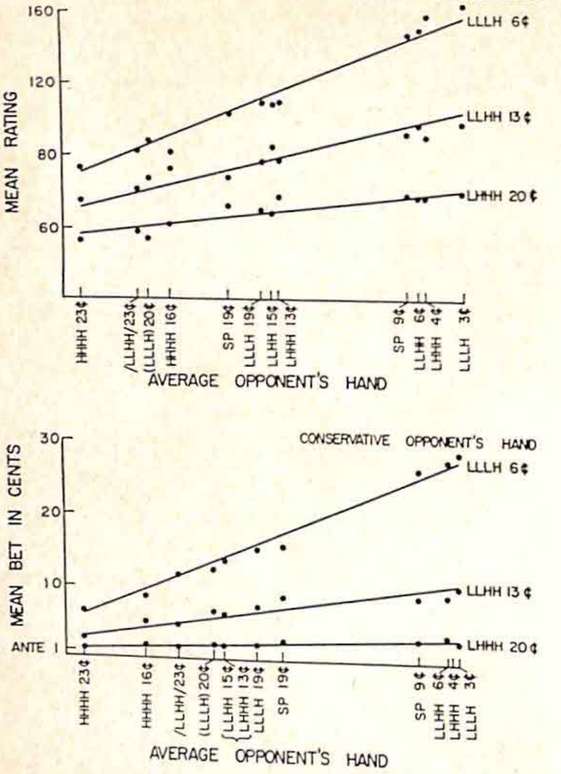

FIGURE 1. Obtained and predicted data for rating and betting conditions of Experiment 1. (Note that the average opponent's hands are spaced along the abscissas in accord with their marginal means. SP = hand showing a small pair; HP = hand showing a high pair; L = card lower or equal to 7; H = card higher than 7; parentheses = possible flush; one slash = possible inside straight; two slashes = possible open-ended straight.)

to experiment, most descriptions contained three items: (a) the four upcards coded by rank and suit. (These cards defined the set of potential opposing hands. The actual opposing hand, of course, depended on the value of the face-down hole card.) (b) the playing style of the opponent, either conservative or average. (The exact meanings of conservative and average were left up to the subjects; they were instructed only that the conservative player would bluff less often than the average player.) (c) the bet-amount, from 1¢ (ante) to 25¢. (This was the total amount the opponent was willing to bet on the hand summed over all rounds of betting.)

In most cases, two other pieces of information were explicitly excluded from the description. First, the subjects did not know why the betting had stopped at the amount given in the description. Thus, the subjects did not know whether the opposing player folded, called, was called, or was the last player in the pot. Second, the subjects did not know what hands the opposing player was looking at when he played the hand, and therefore did not know whether the opponent was betting in the face of opposing strength or not.

An example of a stimulus hand is shown below:

CONSERVATIVE PLAYER

2 4 Q K ?
C D H S

22¢.

The description indicates an opposing hand composed of the 2 of clubs, 4 of diamonds, queen of hearts, king of spades, and a face-down hole card. The hand was played by a conservative opponent who bet a total of 22¢ on the hand.

Stimulus code. Although the subjects saw hands comprised of cards of definite rank and suit, the experimental designs defined the hands abstractly. Basically, there were two types of stimulus hands. (a) Hands that could be at best a pair are coded by a set of Ls and Hs. L designates a card lower than or equal to a 7 (the rank of the standard pair), and H designates a card higher than a 7. Thus, LLHH might designate 249A or 34JQ. (b) Hands that could be better than a pair have special codes. SP designates a hand already showing a small pair, for example, 558K. Parentheses, for example, (LLLH), designate a possible flush; one or two slashes, for example, LLH/H and /LHHH/, designate possible inside and open-ended straights, respectively.

Stimulus pairs. In most of the experiments the stimulus hands were presented to the subjects two at a time, and the subjects were asked to judge the likelihood of beating *both* of the stimulus hands. The stimulus hands were combined into pairs using two-way Conservative-Opponent's-Hand × Average-Opponent's-Hand factorial designs. For illustration, the design for Experiment 1 is shown in the top panel of Figure 1. Each of the 36 stimulus pairs is represented by the intersection of a row (conservative opponent's hand) and a column (average opponent's hand). Although Figure 1 is described in terms of abstract hand codes, the subjects saw concrete stimulus hands. For example, the stimulus pair seventh from the left in the top row might have been:

CONSERVATIVE PLAYER

2 5 6 Q ?
S C H S
6¢

AVERAGE PLAYER

3 4 J A ?
D D C H
15¢.

Note that cards were assigned within stimulus pairs, so that the two hands did not share crucial H cards. For example, the pair 36QA and 25QK would not have been allowed. Similar precautions were taken with hands showing small pairs, possible straights, and possible flushes.

Standard hand. In all the experiments the subjects judged the likelihood of beating the opposing hands with a standard hand: a pair of 7s, 7D and 7H, with the three other cards not specified. This hand was chosen as the standard since it is about midway in the ranks of cards and allows the definition of LLLH and HHHH hands that are not also possible straights. Subjects were instructed that in the event of a tie, their 7s would beat anyone else's 7s.

Hole cards. Since the subjects made real side-bets on the various stimulus pairs, it was necessary to assign a hole card to each stimulus hand. These hole cards were not part of the stimulus design but they were vitally important since, if the hole cards were not realistic in light of the stimulus information, the subjects could not have been expected to perform as poker players.

In general, each experiment was set up so that 40 to 45% of the stimulus pairs were winners for the subjects, and 55 to 60% were losers. Within this restriction, hole cards were assigned according to three heuristics: (a) for both conservative and average opponents, opposing hands with large bets were more likely to beat 7s than opposing hands with small bets; (b) for a constant bet amount, the conservative opponent was more likely to beat 7s than the average opponent; and (c) for a constant bet and holding constant whether the hand beats 7s or does not, the conservative opponent usually held a better hand than the average opponent. These heuristics represent two generalizations that are of little empirical interest in their own right and that were assumed to be part of the basic knowledge of all poker players: First, although bluffing and sandbagging sometimes occur, large bets usually indicate strong hands, and second, although all players must bluff occasionally, conservative players usually require better cards for a given bet than average or risky players.

EXPERIMENT 1

Experiment 1 tested two hypotheses: First, that subjects integrate their likelihoods of beating individual opposing poker hands into a global likelihood of winning by a simple multiplicative process, and second, that they set the amount of a side-bet by betting proportional to their likelihood of winning.

The hypothesized multiplicative integration rule may be expressed mathematically as follows,

$$SL_{CiAj} = (SL_{Ci})(SL_{Aj}), \qquad (1)$$

where SL_{Ci} is the subjective likelihood of beating the conservative opponent's hand, SL_{Aj} is the subjective likelihood of beating the average opponent's hand, and SL_{CiAj} is the subjective likelihood of beating them both. The various *SL*s are assumed to vary between zero and one.

Information integration theory (Anderson, 1974b) provides a simple procedure for testing this hypothesized integration rule. Consider a set of stimulus pairs constructured from a row by column, Conservative-Opponent's-Hand × Average-Opponent's-Hand, factorial design, and suppose that ratings of the likelihood of beating particular stimulus pairs, R_{CiAj}, will be linear with SL_{CiAj} so that:

$$R_{CiAj} = \beta(SL_{Ci})(SL_{Aj}) + \alpha. \qquad (2)$$

If the data are in fact perfectly multiplicative, then for any column,

$$\bar{R}_{C.Aj} = \beta(\overline{SL}_{C.})(SL_{Aj}) + \alpha, \qquad (3)$$

where $\bar{R}_{C.Aj}$ is the mean of the jth column, $\overline{SL}_{C.}$ is the mean of the subjective likelihoods of beating the conservative opponent's hand over all levels of i, SL_{Aj} is the subjective likelihood of beating the jth level of the average opponent's hand, and α and β are scaling constants that relate the subjective response to the objective response scale. Thus, the mean of each column is linear with the subjective likelihood of beating the stimulus hand in that column, and similarly, the mean of each row is linear with the subjective likelihood of beating the stimulus hand in that row.

Taken together, Equations 2 and 3 yield a simple graphical test of fit for the multiplying rule: If the column stimuli are spaced along the abscissa according to their marginal means, then the data should plot as a fan of straight lines. Furthermore, if analysis of variance is applied to the data, there should be a significant interaction concentrated in the Linear × Linear component and a nonsignificant residual interaction.

The test of the hypothesized proportional betting heuristic follows directly from the general test for multiplicativity. Basically, the rule for bets, B, is identical to that for the rating response,

$$\begin{aligned} B_{CiAj} &= \beta'(SL_{CiAj}) + \alpha' \\ &= \beta'(SL_{Ci})(SL_{Aj}) + \alpha', \qquad (4) \end{aligned}$$

except that in this case the values of α' and β' are restricted by the experimental variables: α' must be equal to the amount of the ante, since the player must pay this even when he is sure he will lose, and β' must be equal to the maximum allowable bet minus the ante, since their sum is the amount the player should bet if he is certain of winning.

Method

Experienced poker players twice judged the likelihood of beating pairs of opposing hands with a standard pair of 7s: First, they rated the likelihood directly, and second, they made bets against the pairs of opposing hands.

Stimulus design. Pairs of stimulus hands were produced using the 3 × 12 Conservative-Opponent's-Hand × Average-Opponent's-Hand factorial design shown in the top panel of Figure 1. The stimulus hands were described by the four upcards in the hand, the playing style of the opponent holding the hand, and the amount bet on the hand. Each hand was printed separately on a 4 × 6 index card. A notary seal represented the hole and concealed a strip of paper listing the hole card.

Procedure. The subjects were run individually in two 1-hour sessions. In the first session, the experimenter explained the experimental format and the various stimulus codes. The stimulus hands were presented to the subjects as representative of hands that might occur in real poker games. The experimenter stressed that the hands in each pair had come from different games and had not been played against each other, and that the subject would not be told what hands the opponents were looking at when they played the stimulus hands or why the betting had stopped at the amounts shown on the stimulus cards. The subject's task was to rate how likely he would be to beat both of the opposing hands with a pair of 7s. The ratings were given on a 200-mm graphical response scale that ran from "certain to win" to "certain to lose." Each of the 36 experimental pairs was rated twice. At no time during the rating session were the hole cards of the experimental pairs revealed.

In the second session subjects saw the same 36 stimulus pairs. This time they responded to the pair by making a side-bet with the experimenter on whether they could beat both opposing hands with a pair of 7s. The dependent variable was the amount the subject chose for the side-bet, from a 1¢ ante to a 30¢ maximum. If the subject won the bet, the experimenter would pay him the amount of the bet; if he lost, he would pay the experimenter the amount of the bet. Each subject provided side-bets for four replications. After each replication, six stimulus pairs were selected at random, the hole cards were revealed, and the bets were paid off or collected.

Subjects. The subjects were 10 male students at the University of California, San Diego, who replied to a newspaper advertisement asking for poker players. For Session 1, the subjects were paid a flat rate of $1.88. For Session 2, the subjects were paid $1 plus winnings, which varied between 25¢ and $2 depending on the luck and skill of the subject.

Results

The results for the rating and betting conditions of Experiment 1 are shown, respectively, in the top and bottom panels of Figure 1. In both panels the average opponent's hand is spaced along the abscissa according to marginal means. The dots are data and the lines are best-fitting predictions from Equations 2 and 4.[2]

Graphically (see Figure 1), the multiplying rules seem to fit the data quite well: The average absolute deviations between predicted and obtained are 3.80 mm (1.06 standard errors) for ratings and .66¢ (1.18 standard errors) for bets. To confirm this good fit, individual analyses of bilinearity were performed for each subject for both rating and betting conditions. The results are given in Table 1. For the rating data, 9 of the 10 subjects show highly significant bilinear interactions. However, of these 9 subjects, 3 show small but statistically significant residual interactions that are not consistent with a strictly multiplicative model. For the bet data, the results are similar: All 10 subjects show large and significant bilinear interactions, but 4 also show significant residual interactions.

The cause of the significant residuals is not readily apparent. For the most part, graphical inspection of the interactions revealed only unsystematic deviations from bilinearity, which might reflect no more than minor procedural problems, such as limited practice or number biases in the bet response. There were two exception however. For Subjects 6 and 10 there were small end effects in the bet condition: The last four data points in the top row were all at the maximum response of 30¢.

Discussion

Ratings. The rating data bear directly on the process for integrating information from two opposing stimulus hands. They tend to support the hypothesized multiplicative rule, and they are consistent with earlier research by Shuford (1959) on subjective probabilities for compound events.

TABLE 1

LINEAR × LINEAR ANALYSIS FOR EXPERIMENT 1

Subject	Rating data		Bet data	
	Bilinear $F(1, 36)$	Residual $F(21, 36)$	Bilinear $F(1, 108)$	Residual $F(21, 108)$
1	49.36*	1.01	312.71*	.37
2	45.56*	1.35	308.24*	.66
3	10.63*	.72	39.24*	1.02
4	18.63*	2.37*	133.10*	1.04
5	14.37*	2.93*	70.44*	.89
6	41.04*	1.30	107.99*	2.04*
7	74.78*	1.21	230.33*	2.09*
8	11.44*	1.68	23.13*	1.87*
9	.20	.93	140.66*	.66
10	109.65*	1.96*	207.51*	4.70*

* $p < .05$.

The data also bear indirectly on the process for evaluating subjective likelihoods. The good fit of the multiplicative rule implies that opposing stimulus hands do not interact with one another during evaluation; that is, evaluation of the likelihood of beating a particular opposing hand is not affected by the context of the opposing hand with which it appears. This fact has two important consequences: First, it will simplify the experimental analysis of the evaluation process, and second, it demonstrates the unity and robustness of the subjective likelihoods that result from stimulus evaluation.

Bets. The bet condition required both evaluation and integration of information from two opposing hands. However, the required response was a bet amount that did not constrain the evaluation and integration processes to be identical with those in the rating task. In Equation 4, the evaluation and integration processes are

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[2] Best-fitting predictions for multiplicative processes can be obtained using marginal means provided one knows the value of α, the additive constant. For the bet data, α was constrained to be 1; no similar constraint existed for the rating data, so α was estimated visually to be 25 mm. Given these estimates of α, best-fitting predictions from the models were derived using the marginal means of the data according to the equation:

$$P_{CiAj} = \frac{(\bar{R}_{C.Aj} - \alpha)(\bar{R}_{CiA.} - \alpha) + \alpha}{(\bar{\bar{R}}_{C.A.} - \alpha)},$$

where P_{CiAj} is the prediction, $\bar{R}_{C.Aj}$ and $\bar{R}_{CiA.}$ are the marginal means, and $\bar{\bar{R}}_{C.A.}$ is the grand mean.

assumed to be the same as for the rating task, and the bet itself is assumed to be a simple linear function of the subjective likelihood of winning. This implies that the data should be practically identical for the two conditions. However, three other possibilities exist:

1. The bet could be some nonlinear function of subjective likelihood, in which case the data would not be bilinear. As an example, one might maximize expected value, betting the maximum whenever SL_{CiAj} is greater than .5 and betting the minimum whenever SL_{CiAj} is less than .5.
2. The integration rule for betting could be nonmultiplicative. For example, one might attend only to the more threatening hand.
3. The evaluation process might differ for bets producing different subjective likelihoods under the two conditions.

The data of Figure 1 bear directly on the first two possibilities. The overall similarity between the two panels provides provisional support for the hypothesis that the multiplicative integration rule holds for bets as well as for ratings, and the small within-cell standard deviation for single subjects (3.45¢) confirms that the result holds for the individual replication and does not reflect averaging over some all-or-none process. Furthermore, the same bet-rating similarity also supports the hypothesis that the bet-amount is a simple linear function of the subjective likelihood of winning.

The third possibility can be examined using functional measurement (Anderson, 1970). Having obtained provisional support for multiplicative integration rules for both bets and ratings, the marginal means of the data can be used as interval level estimates of the subjective likelihoods of beating the various stimulus hands (see Equation 3), and these can be compared directly across the two conditions to see whether differences have occurred.

In Figure 1, the marginal means for the average opponent's hands are represented by the spacing of the hands along the two abscissas. Overall, there is good correspondence between the two sets of means: The correlation between them is +.96. Thus, it is unlikely that the evaluation process is much different for betting than for rating. However, sizable differences do occur in spacing and even ordering for a few of the hands, and these probably reflect real differences in the relative importance of information under the two response conditions, as has been demonstrated in another context by Lichtenstein and Slovic (1973).

EXPERIMENT 2

Evaluation of the subjective likelihood of beating an opposing hand requires at least two subprocesses:

1. Each stimulus item must be interpreted in terms of the extent to which it implies a strong or a weak hand. Such interpretation may be simple or not, depending on whether the information is directly scalable on the dimension of beatableness. For example, the raw probability information provided by the upcards bears directly on beatableness. In contrast, the playing style of the opponent is not directly related to beatableness but, instead, modifies the meaning of other information such as bet amount.
2. After the stimulus items have been interpreted, they must be combined into the overall subjective likelihood of beating the hand. This could involve any of several possible combination rules: adding, averaging, or multiplying, for instance.

Experiment 2 was designed to provide preliminary data on these subprocesses.

Method

Experienced poker players judged the likelihood of beating single opposing hands by making bets that the hands could be beaten by a pair of 7s.

Stimulus design. Each opposing hand was described by the four upcards in the hand, the playing style of the opponent holding the hand, and the amount bet on the hand. Five different stimulus replications were constructed for the 2 × 4 × 7 Opponent-Style × Bet-Amount × Upcards factorial design shown in Figure 2. The stimulus hands were printed on 4 × 6 index cards identical to those of Experiment 1 and were presented to the subjects one at a time.

Procedure. The instructions for Experiment 2 were similar to those for Experiment 1. The experimenter described the various types of information in the stimulus hands and instructed the subject that his task would be to make bets between 1¢ and 25¢[3] as to whether he could beat single opposing hands with the standard pair of 7s. The hands from the risky opponent were played first and then the hands from the conservative opponent. After bets were complete for each replication, a sample of hands was selected at random, the hole cards were revealed, and the bets were paid off or collected.

Subjects. The subjects were 11 male students at the University of California, San Diego, who replied to a newspaper advertisement asking for poker players. They were paid a flat rate of $1 plus winnings, which varied between 50¢ and $3 depending on the luck and skill of the subject.

Results

The results of Experiment 2 are shown in Figure 2. The data for the risky and conservative opponents are in the left and right panels, respectively. The upcards are listed along the abscissa in increasing order of objective probability of beating a pair of 7s. The row parameter is the amount bet on the hand by the opposing player. The data for 18–20¢ bets and 23–25¢ bets are pooled, since there was no significant difference between them, $F(1, 180) = .45$.

Visual inspection of Figure 2 reveals that all three factors influenced the subjects' bets: The mean bet varied inversely with the conservativeness of the opponent-style, $F(1, 10) = 59.63$, and the amount of the opponent's bet, $F(3, 30) = 81.62$, and nonmonotonically with the objective threat of the upcards, $F(6, 60) = 14.45$. In addition to the main effects, there were three significant interactions. The Opponent-Style × Bet-Amount interaction, $F(3, 30) = 3.33$, appears in Figure 2 as the difference in spacing between the corresponding rows of the two panels. The Bet-Amount × Upcards interaction, $F(18, 180) = 2.06$, appears as the nonparallelism of the rows within each panel. The Opponent-Style × Bet-Amount × Upcards interaction, $F(18, 180) = 1.90$, appears as the slope difference between corresponding rows of the two panels and reflects, at least in part, a floor effect for the conservative opponent.

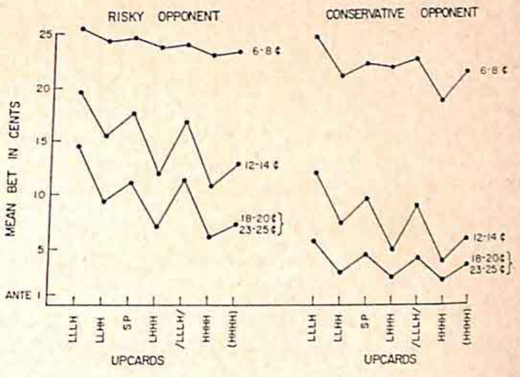

FIGURE 2. Mean bet data for risky and conservative conditions of Experiment 2. (See Figure 1 caption for the definitions of the abbreviations used.)

Discussion

Nonmonotonic processing. The data of Experiment 2 are most striking for the nonmonotonic relationship between the objective threat of the upcards and the amount of the subject's bet. For the possible pairs, LLLH, LLHH, LHHH, and HHHH, the mean bet decreases monotonically with the objective probability that the hand is better than a pair of 7s. However, nonmonotonic increases occur for the small pair, SP; the possible straight, /LLLH/; and the possible flush, (HHHH).

Either or both of two mechanisms could account for this data. First, the nonmonotonicity could reflect a bias in the subject's model of the mathematical structure of the game. Two types of probability are important in stud poker: Simple probabilities are relevant before the deal, and conditional probabilities are relevant after. For example, the simple probability of dealing a flush is 5,108/2,598,960 or .002. However, once four to a flush have been dealt, the conditional probability of filling the flush is 9/48 or .19. It may be, therefore, that subjects fail, at least in part, to consider the larger conditional probabilities when the upcards allow the possibility of rare hands, like flush, straight, two-pair, or three of a kind.

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[3] The side-bet range was intended to be 1¢ to 25¢. Inadvertently, however, four subjects used a 1¢ to 30¢ range. This small difference in response scale does not bear on any of the results to be discussed and will be ignored.

Second, the nonmonotonicity could reflect active strategic discounting based on the subject's model of the opponent. Bluffing is central in poker. Every player must bluff at least occasionally and must also watch for bluffs from his opponents. Suppose that the player incorporates this generality into a model of the following sort: The opponent's bet is assumed to vary directly with the actual strength of the hand and the bluffableness or potential strength of the hand, so that opponent's bet = (hand strength) + (hand bluffableness). Since the subject knows the amount of the opponent's bet and the potential strength of the opponent's hand, he can approximate a solution for the actual strength of the opponent's hand. This could produce the nonmonotone pattern of Figure 2. For example, since LHHH can be at best a pair, it has relatively low bluffableness value: It warrants a high bet only when its actual strength is high, that is, when it is in fact a pair. In contrast, /LLLH/ can be at best a straight and therefore has high bluffableness value: It warrants a high bet even when its actual value is negligible. Thus, if the bet-amount is held constant, the subject should approximate the actual hand strength to be greater for LHHH than for /LLLH/ and should, therefore, bet less against LHHH than against /LLLH/.

Monotonic processing. Figure 2 emphasizes the nonmonotonic relationship between the objective threat of the upcards and the subjects' bets. However, the data also reveal a monotonic relationship between upcards and bets: Within each basic hand type, that is, hands that could be at best a pair and hands that could be better than a pair, the mean bet is a decreasing function of the objective threat of the upcards. For example, pooling over opponent-style, the means for LLLH, LLHH, LHHH, and HHHH are 15.30¢, 11.58¢, 10.21¢, and 9.06¢, respectively, and the means for SP, /LLLH/, and (HHHH) are 13.16¢, 12.90¢, and 10.55¢, respectively.

This monotonic relationship is especially interesting if the nonmonotonicity in Figure 2 reflects a bluff-discounting process. Hands that have high bluffability allow the possibility of rare and powerful hands. However, in most cases, these same hands also have high objective likelihood of being good hands. For example, (LHHH) is a high-bluffable hand, since it allows the possibility of a flush, but it is also objectively threatening, since there are 18 cards in the deck that will make it better than a pair of 7s. Thus, the subject must reconcile bluffableness information that decreases threat with probability information that increases threat.

In this regard, the data provide some evidence about how subjective threat and bluffableness grow with objective threat. LLLH is a low-threat, low-bluffable hand: Adding a possible straight, /LLLH/, increases both threat and bluffableness. Since the mean bet is greater for LLLH than for /LLLH/, the increase in subjective threat must have been greater than the increase in bluffableness. In contrast, HHHH is a high-threat, low-bluffable hand; adding a possible flush, (HHHH), again increases both threat and bluffableness. Now, however, the mean bet is greater for (HHHH) than for HHHH, indicating that the increase in bluffableness must have been greater than the increase in subjective threat. Assuming that there is no important difference between possible straights and possible flushes, the data imply at least that the subjective threat function increases to asymptote more quickly than the bluffableness function.

Scalar processing. In Figure 3 the data are pooled over upcards to show the significant Opponent-Style × Bet-Amount interaction. Bet-amounts are spaced along the abscissa according to marginal means. The dots are data; the lines are predictions from the linear regressions of the data for conservative style and risky style on the marginal means.

Two features of the data deserve comment. First, the data plot as approximately multiplicative functions of the marginal means for bet-amount, suggesting that playing style is used to scale the actual bet-amounts into subjective units. Scalar processes of this type are potentially important as general mechanisms for integrating information that does not lie on a common dimension. Second, assuming a scalar process, the marginal means of the bet-amounts are functional scale values of the subjective bet-amounts. As might be predicted from classical utility curves, these are a decreasing function of actual bet-amount.

EXPERIMENT 3

Experiment 3 has four parts: Part 1 replicates the test of the multiplicative rule for the two-hand integration task, Parts 2 and 3 test the mathematical bias and the bluff-discounting hypotheses for the nonmonotonicity in Figure 2, and Part 4 tests the rule describing the combination of upcard and bet-amount information in the evaluation process.

Part 1: The Multiplicative Integration Rule

The data from Experiment 1 generally supported a multiplicative rule for the two-hand integration task. However, there were several significant deviations from the rule at the individual subject level. Part 1 of Experiment 3 replicates the bet condition of Experiment 1 with four procedural improvements: (a) practice was increased, (b) high and low anchor stimuli were added, (c) stimulus replications replaced fixed stimulus hands, and (d) replications were increased.

Part 2: Mathematical Biases in Subjective Likelihoods

The hypothesis that subjects underestimate the conditional likelihood of rare hands predicts that the discrepancies between the subjective and objective likelihoods of beating potentially rare hands will be more positive than the discrepancies between the subjective and objective likelihoods of beating potentially common hands. This prediction can be tested directly provided that two conditions are met:

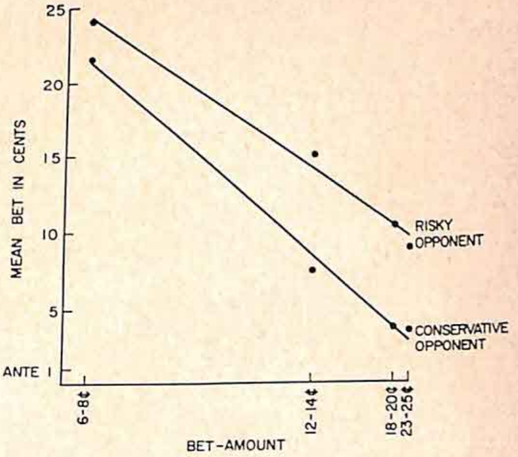

FIGURE 3. Mean bet data for Experiment 2 pooled over upcards.

1. The effects of the hypothesized mathematical bias must be isolated from the potentially confounding effects of bluff-discounting. This can be accomplished by using hands like:

3 6 Q A ?
S S D C

NO DATA,

which specify only upcard information. For these hands, hole cards are assigned at random. If the card produces a hand that beats 7s, the subjects lose; if not, they win. Thus, such hands require evaluation of only the objective threat of the upcards.

2. The measures of subjective likelihood must be on an interval or ratio scale. This can be achieved by using the two-hand task as a vehicle for scaling the subjective likelihoods of beating single hands. The hands to be scaled, for example, the set of NO DATA hands, are assigned as the levels of one factor, the *target factor*. These are combined factorially with hands from a *dummy factor*, whose purpose is to provide a test of the multiplicative integration rule. If the data demonstrate the necessary bilinear interaction, the marginal means for the target factor are

interval scale measures of the subjective likelihoods of beating the target hands (see Equation 3). Furthermore, if the target factor contains anchor hands that can be assigned the values zero and one, the marginal means can be transformed to ratio scale measures of subjective likelihood.

Part 3: Strategic Discounting of Bluffable Hands

As was noted above, the bluffableness of a poker hand tends to be confounded with objective threat. This makes it impossible to test the bluff-discount hypothesis by direct comparison of responses to high- and low-bluffable hands. Fortunately, another test is possible.

Ordinarily, a poker player has three betting options: He can call, raise, or fold. However, if the player is bluffing, he can only raise or fold, since a call would require him to show his presumably losing hole card. Therefore, a call implies that a player is not bluffing.

The test of the bluff-discount hypothesis rests on the differential impact of a call depending on whether the opponent is likely to be bluffing or not. If the opponent bets a moderately large amount on a low-bluffable hand, for example:

AVERAGE PLAYER

3 5 8 Q ?
H C C H

17¢,

it is likely that he has something, usually a pair. The information that his bet represents a call, as in:

AVERAGE PLAYER

3 5 8 Q ?
H C C H

17¢
CALLED,

is superfluous, and there should be little difference in the subjective likelihoods of beating the two forms of the hand. However, if the opponent bets the same amount on a high-bluffable hand, for example:

AVERAGE PLAYER

2 4 J K ?
S S S S

17¢

he might be bluffing. The information that his bet represents a call, as in:

AVERAGE PLAYER

2 4 J K ?
S S S S

17¢
CALLED,

is highly meaningful, since it eliminates the possibility of a bluff. Thus, it should produce a significant reduction in the subjective likelihood of beating high-bluffable hands.

Note that this test also requires interval or ratio level measures of subjective likelihood. These can be obtained using the two-hand functional measurement procedure outlined above.

Part 4: The Combination of Bet-Amount and Upcards

Both bet-amount and upcards convey information about the likelihood that an opposing hand can beat a pair of 7s. This information can be concordant as in:

2 4 5 Q ?
S C D D

3¢,

or it can be contrary as in:

2 4 5 Q ?
S C D D

22¢.

Whatever the information, the player must combine it to arrive at an overall evaluation of the likelihood of beating a hand.

The particular combination rule a player uses is part of his model of the game and reflects certain basic assumptions about the structure and meaning of the available information. In general, such combination rules are divisible into two categories: algebraic rules and configural rules.

Algebraic rules. In an algebraic rule, each item of stimulus information is assigned a scale value along a relevant dimension, and then the individual scale values are combined according to some simple algebraic process. In the case of bet-amount and upcards, three different algebraic processes are defensible on psychological grounds. These are averaging, adding, and multiplying:

1. In an averaging process, the player considers bet-amount information and upcard information to provide independent estimates of the general beatableness of the hand. To the extent that the estimates disagree, compromise is required and the overall evaluation of beatableness will lie of necessity between the component evaluations.

2. In an adding process, the player considers bet-amount information and upcard information to bear on different aspects of beatableness. Consequently, there is no reason to expect agreement between the items, and the overall evaluation of beatableness can lie either between or outside the component evaluations, depending on the particular situation.

3. In a multiplying process, the player considers bet-amount information and upcard information to bear on the general beatableness of the hand, just as in averaging. However, the dimension of evaluation is not simple beatableness, but is specifically the subjective probability of beating the hand, given bet-amount or upcards. In this case, overall evaluation requires a multiplicative process: To the extent that either component evaluation is low, the overall subjective probability of beating the hand must also be low.

Configural rules. The essence of a configural rule is that stimulus meaning is not constant, but varies with context. Configural stimulus interactions range from completely idiosyncratic effects for particular items to systematic effects like assimilation and contrast. In the case of bet-amount and upcard information, one type of configural process is particularly plausible. This is an optimal matching rule: The player considers the set of possible hand types defined by the upcards and decides which hand is most likely to have produced the given bet-amount. For example,

2 6 9 A ?
H H H H

10¢

can produce an ace-high no-pair, pairs of 2s, 6s, 9s, or aces, or a flush. A player using optimal matching may consider a 10¢ bet to be too small for a flush, too large for a no-pair, and just right for an intermediate pair like 6s or 9s.

Experimental tests. Part 4 of Experiment 3 was designed principally to test between averaging and adding processes for the combination of bet-amount and upcards. If responses to NO DATA hands, ranging from low threat to high threat, are compared graphically with responses to the same upcards combined with an intermediate bet-amount, averaging and adding processes are clearly distinguishable. In an adding process, the NO DATA curve will lie either entirely above or entirely below the intermediate bet curve, depending on whether the subjective value of the particular bet-amount is slightly positive or slightly negative. In an averaging process, a crossover will result: For low-threat upcards, the intermediate bet-curve will lie below the NO DATA curve, whereas for high-threat upcards, the intermediate bet curve will lie above the NO DATA curve.

The averaging–adding test is qualitative and requires only ordinal response measures. In contrast, testing between adding and multiplying calls for interval or ratio scale data. Often such data are not available. However, in the present case, the two-hand functional measurement procedure can be used to get ratio scale measures of the subjective likelihoods of beating the various hands, and these can be used to distinguish adding and multiplying. In an adding process, the NO DATA curve will be parallel with the intermediate bet curve; in a multiplying process, the NO DATA curve and the intermediate bet curve will converge toward the high-threat upcards.

Method

For each of the four parts of Experiment 3, experienced poker players judged the likelihood of beating pairs of opposing hands by making bets that the hands could be beaten by a pair of 7s.

Stimulus designs. The stimulus designs for the four parts of the experiment were each two-way Average-Opponent's-Hand × Conservative-Opponent's-Hand factorial designs. (The codes for the particular stimulus hands are shown in Figures 4 through 7.) Taken as a group, the designs included four types of stimulus hands: (a) standard hands, composed of upcards, opponent-style, and bet-amount; (b) CALLED hands, which are standard hands with "CALLED" added under the bet-amount to indicate that the bet represents a call; (c) NO DATA hands, composed of upcards and the words "NO DATA" to indicate that the hole card has been assigned at random; and (d) scaling anchors, which define the zero and one points on the dimension of subjective beatableness. The low scaling anchor is an unbeatable pair higher than 7s (HP) combined with a 23¢ bet-amount. The high scaling anchor is an LLLH hand combined with a 1¢ bet-amount.

Each of the designs allows both a test of the multiplicative integration rule and functional scaling of the stimulus hands. For Part 1, the objective was the test of the integration rule; for Parts 2, 3, and 4, the objective was the functional scaling of sets of target hands.

Stimulus sets. Five different stimulus replications were constructed for each of the four basic designs. Then pairs that included only standard hands, NO DATA hands, and scaling anchors were intermixed to form five stimulus sets. These were designated *no-cause* sets, since for each pair, neither hand specified why the betting had stopped. Pairs that included CALLED hands were intermixed with pairs from a pilot study not reported here to form five other stimulus sets. These were designated *cause* sets, since the average opponent's hand specified why he had stopped betting. In addition to CALLED, the causes given in the pilot study included: FOLDED, indicating that the opponent had folded after one or more rounds of betting; WAS CALLED, indicating that one or more players had called the opponent; and LAST IN POT, indicating that all players except the opponent had dropped out of the pot.

Warm-up pairs were added at the beginning of each stimulus set and response anchors were distributed throughout the set. Counting these, the no-cause sets each consisted of 150 stimulus pairs, including 126 experimental pairs; the cause sets each consisted of 100 stimulus pairs, including 24 experimental pairs.

Procedure. Experiment 3 required approximately 15 hours per subject and was run in 12 sessions spanning a period of 3 to 4 weeks. The subjects were run in small groups for Session 1 and individually for Sessions 2 through 12. In the first session, small groups of subjects and the experimenter played the modified game for about 1 hour. The second session was devoted to instructions and practice. For Sessions 3 through 7, the subject played two different no-cause sets each day, and for Sessions 8 through 12, the subject played two different cause sets per day. This gave 10 replications per subject for each of the four designs.

The stimuli were presented to the subject on a Video Systems Corp. (Model 2000) video terminal interfaced to a PDP-12 computer. The subject responded by typing the amount of his bet on a keyboard located immediately beneath the screen. During the experimental sessions, approximately 25% of the hands were chosen at random, and the hole cards were revealed. The winner was specified, and the computer kept track of the subject's wins and losses.

Hole cards. The test of bluff-discounting required that subjects' responses reflect their models of the game and the opponent, and not the payoff structure of the experiment. For this reason, winning and losing hole cards were assigned throughout the experiment in accord with the size of the bet and the playing style of the opponent but *independent* of the upcards. For example, for hands where the conservative opponent bet 15¢, hole cards were distributed rectangularly across upcard types, so that the conservative opponent won 50% of the hands and lost 50% of the hands. Similarly, when the average opponent bet 15¢, hole cards were distributed rectangularly across upcard types, so that the average opponent won 40% of the hands and lost 60%.

In the cause condition, CALLED information increased the probability of the opponents beating 7s by .20 evenly across all bet-amounts, and FOLDED information decreased the probability evenly by .20. For WAS CALLED and LAST IN POT hands, the probabilities were identical to those for standard hands. For the NO DATA hands, hole cards were assigned so that the proportion of winners and losers over all instances would be approximately equal to the expected proportion.

Subjects. The subjects were seven male students at the University of California, San Diego, who responded to an advertisement for poker players, and who passed a simple poker examination. The subjects were paid a flat rate of $1 per hour plus the amount of their winnings. The final hourly pay for the 12 sessions varied between $1.51 and $2.46 depending on the luck and skill of the subject.

Results and Discussion

Part 1: The multiplicative integration rule. Figure 4 shows the results for Part 1 pooled over subjects and replications. The dots are data and the lines are predictions from the marginal means of the group data (see Footnote 2). Overall the multiplicative rule seems to fit the data quite well.

To confirm this good fit, the individual data for each of the subjects were tested for bilinearity. These results, given in Part 1 of Table 2, strongly support the multiplicative integration rule: In every case, the Linear × Linear component is highly significant, and the residual is nonsignificant.

The results also support the hypothesis that the subject sets his bet proportional to his subjective likelihood of winning. This is interesting from a functional point of view. In one way, the procedure is non-optimal: The subject could increase his expected gain per gamble by using a maximizing strategy. However, proportional betting yields a smaller expected variance per gamble than maximizing, and variance has often been cited as a major determinant of the subjective risk of a gamble (Coombs & Meyer, 1969; Pollatsek & Tversky, 1970; Rapoport & Stein, 1974). Thus, by betting proportional to his subjective likelihood of winning, the subject may be minimizing subjective risk.

Parts 2–4: Bilinear tests and functional scaling. The data for the Dummy-Factor × Target-Factor designs of Parts 2, 3, and 4 were tested for bilinearity at the level of the single subject. The results are given in Parts 2, 3, and 4 of Table 2. In every case the Linear × Linear components are significant, and in 19 cases the residuals are not. The remaining two residuals are small and appear graphically to be nonsystematic. Since this is about what one would expect by chance, the results were taken as confirming bilinearity and permitting functional scaling of the various target hands. (Incidentally, these good bilinear results argue that NO DATA and CALLED hands are comparable to standard hands in terms of their effect on the two-hand task. Although NO DATA and

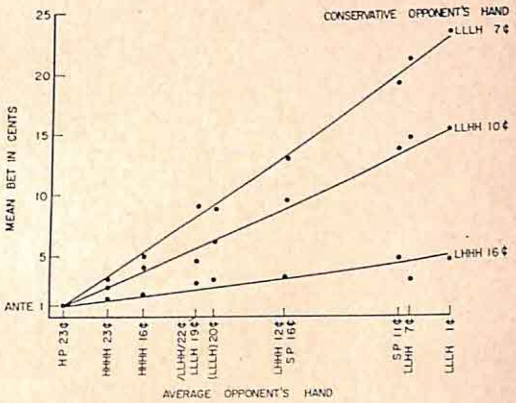

FIGURE 4. Obtained and predicted data for Part 1 of Experiment 3: Test of the multiplicative integration rule. (Note that the average opponent's hands are spaced along the abscissa in accord with their marginal means. See Figure 1 caption for the definitions of the abbreviations used.)

TABLE 2

RESULTS OF INDIVIDUAL BILINEAR ANALYSES FOR EXPERIMENT 3

	Part 1		Part 2		Part 3		Part 4	
Subject	Bilinear $F(1, 270)$	Residual $F(17, 270)$	Bilinear $F(1, 270)$	Residual $F(17, 270)$	Bilinear $F(1, 486)$	Residual $F(33, 486)$	Bilinear $F(1, 243)$	Residual $F(15, 243)$
1	84.27*	.80	155.34*	1.24	745.17*	2.96*	170.70*	1.24
2	41.32*	1.09	10.51*	.83	78.74*	1.01	4.26*	.92
3	18.43*	.35	4.41[a]*	1.15[a]	51.27*	1.07	13.65*	1.36
4	128.37*	1.16	23.93*	1.51	75.24*	1.27	21.01*	1.03
5	268.48*	.70	388.27*	.81	400.87*	.70	109.45*	1.84*
6	158.58*	.53	5.96[a]*	.88[a]	226.87*	1.30	207.05*	.64
7	105.46*	.32	28.52*	.66	252.36*	.87	75.66*	.31

Note. The analyses reported here exclude the low scaling anchor, since it produces errorless data for the stimulus pairs in which it occurs. Analyses were also run that included the low anchor, but they did not differ in any important respect from those above.

[a] The top row of data for Subjects 3 and 6 in Part 2 showed strong ceiling effects. Since the purpose of Part 2 was to get functional scale values for the stimuli and not to test the multiplicative model per se, the lower two rows of data were tested for bilinearity. The results are as shown on $df = (1, 180)$ for bilinear and $(8, 180)$ for residual.

* $p < .05$.

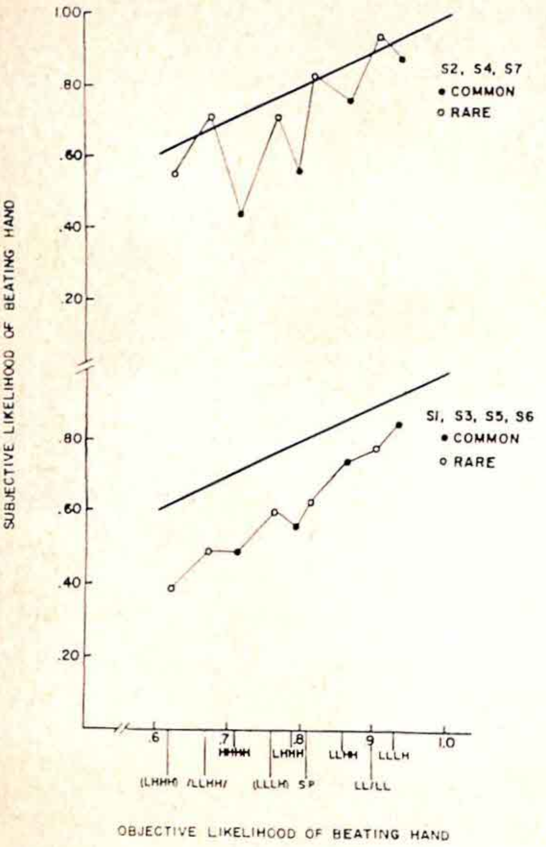

FIGURE 5. Data for Part 2 of Experiment 3: Test of the mathematical-bias hypothesis. (See Figure 1 caption for the definitions of the abbreviations used.)

CALLED hands seem superficially to differ greatly from standard hands, the statistical analyses of bilinearity revealed no qualitative differences between the three hand types.)

Functional scales were derived for each subject individually by linearly transforming the marginal means of each target factor, so that the subjective likelihood of beating the high scaling anchor equaled 1.00, and the subjective likelihood of beating the low scaling anchor equaled .00. This was done separately for each of the 10 replications in order to provide an error term for subsequent single-subject analysis.

Part 2: The mathematical bias hypothesis. The results for Part 2 are shown in the two panels of Figure 5. The data are the functionally scaled subjective likelihoods of beating the NO DATA hands listed on the abscissa; the line is the identity function where subjective likelihood equals objective likelihood.[4] If mathematical bias occurs, then the subjective–objective difference will be more positive for the potentially rare hands, LL/LL, SP, (LLLH), /LLHH/, and (LHHH), than for the potentially common hands, LLLH, LLHH, LHHH, and HHHH.

The differences between subjective and objective likelihood were computed separately for each subject for each replication and were submitted individually to analyses of variance. Two tests are of interest: (a) a planned comparison of rare versus common hands and (b) an overall test of the mean difference (see Table 3).

The results of the planned comparison are mixed. Subjects 2, 4, and 7 show significantly more positive differences for rare hands than for common hands (see top panel in Figure 5); the remaining four subjects show approximately equal differences (see bottom panel in Figure 5). This provides limited support for the hypothesis that the nonmonotonicities in Figure 2 reflect a tendency for subjects to underestimate the conditional likelihoods of rare hands. However, some nonmonotonicity was apparent in the raw data for all 11 subjects in Experiment 2, so the possibility remains that part of the nonmonotone effect may be due to bluff-discounting.

The *F* ratios for the mean difference (see Table 3) show that Subjects 1, 4, 5, 6, and 7 tend significantly to underestimate the likelihoods of beating all hands, as is apparent in Figure 5 in the tendency of the scaled data to fall well below the identity line. This underestimation is interesting in light of previous research on subjective probability. Although several studies (Attneave, 1953; Cohen, 1960; Cohen, Dearnaley, & Hansel, 1956) have

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[4] The objective likelihood of beating a NO DATA hand is simply the number of possible hole cards that would produce a hand poorer than a pair of 7s divided by 42 (the total number of cards unaccounted for in the stimulus display).

TABLE 3

PLANNED COMPARISONS FOR PARTS 2, 3, AND 4 OF EXPERIMENT 3

	Part 2[a]				Part 3[b]			Part 4[c]
	Mean discrepancy (subjective − objective)		F_ψ (rare versus common)		Mean difference (standard − CALLED)		F_ψ (high versus low)	$F_{\text{difference}}$ (NO DATA − standard)
Subject	Common	Rare		F_{mean}	Low bluffable	High bluffable		
1	−.36	−.40	3.20	1,249.62*	.12	.06	7.20*	23.87*
2	−.11	.06	12.00*	.37	—	—	—	.61
3	−.03	.00	.16	.18	.00	.19	8.31*	.34
4	−.24	−.04	16.00*	25.92*	.00	.00	.00	.05
5	−.23	−.23	.00	429.75*	.09	.04	2.50	12.24*
6	−.03	−.08	.86	4.37*	.13	.39	22.53*	3.80
7	−.13	−.04	5.06*	16.68*	.06	.13	1.88	1.17

[a] For both F_ψ and F_{mean}, $df = (1, 81)$. $F_{.05} \simeq 3.96$.

[b] For F_ψ, $df = (1, 72)$. $F_{.05} \simeq 3.98$. The data for Subject 2 were excluded from analysis in spite of good bilinear results. For this subject, and contrary to expectation, the subjective likelihoods for CALLED hands were uniformly higher than for standard hands. The only evidence that bears on this result is anecdotal. Subject 2 remarked that he liked the cause sets and felt that the increased information increased his confidence in betting. This might have produced a response bias toward larger bets for cause pairs in general, and consequently for CALLED hands as well.

[c] For F_ψ, $df = (1, 36)$. $F_{.05} = 4.11$.

* $p < .05$.

shown that subjects tend to underestimate the probability of more likely events and overestimate the probability of less likely events, the present data appear graphically to indicate that the magnitude of the underestimation actually increases as the likelihood of beating the hand decreases.

Part 3: The bluff-discounting hypothesis. The results for Part 3 are shown in the three panels of Figure 6. The eight sets of upcards are listed along the abscissa in order of increasing objective threat. Within each panel, the open circles represent the subjective likelihood of beating the CALLED hands, and the closed circles represent the subjective likelihood of beating the standard (no-cause) hands. If bluff-discounting occurs, the CALLED–standard difference will be greater for the high-bluffable hands, SP, (LLHH), /LLHH/, and (HHHH), than for the low-bluffable hands, LLLH, LLHH, LHHH, and HHHH. (Notice that the solid lines are directly analogous to the lower rows of Figure 2; thus, the nonmonotone pattern should appear in each of the panels of Figure 6.)

The differences between homologous CALLED and standard hands were computed separately for each subject for each replication and were submitted individually to analyses of variance. The results of a planned comparison for high- versus low-bluffable hands are given in Table 3. Only Subjects 3 and 6 had significantly larger differences for high-bluffable hands. The differences for Subject 1 were significantly smaller, probably due to a floor effect; his data were grouped with the subjects who showed no significant effect.

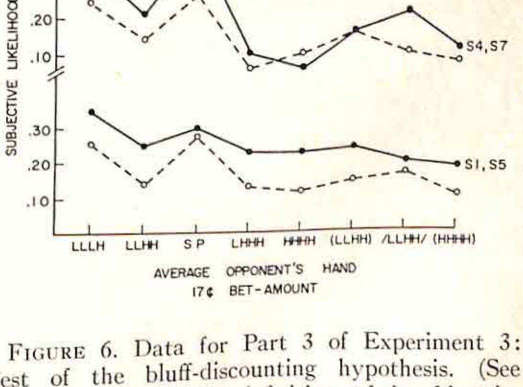

FIGURE 6. Data for Part 3 of Experiment 3: Test of the bluff-discounting hypothesis. (See Figure 1 caption for the definitions of the abbreviations used.)

For Subjects 3 and 6 in the top panel of Figure 6, the evidence of bluff-discounting is striking: There is a strong nonmonotone pattern for the no-cause hands and an equally strong differential effect of CALLED information for the CALLED hands. For Subjects 4 and 7 in the middle panel of Figure 6, there is no bluff-discounting. Although the no-cause data are moderately nonmonotone, approximately the same nonmonotonicity occurs for the CALLED data. Finally, for Subjects 1 and 5 in the bottom panel of Figure 6, there is also no bluff-discounting. However, in this case, there is little effect to account for, since the no-cause data are monotone decreasing for all hands except the small pair.

These results are interesting in conjunction with the results of Part 2. Altogether, four subjects show the nonmonotone pattern for standard hands: Subjects 3, 4, 6, and 7. Of these, Subjects 4 and 7 significantly underestimate the likelihoods of rare hands but do *not* show bluff-discounting. Contrarily, Subjects 3 and 6 do not underestimate the likelihood of rare hands but *do* demonstrate significant bluff-discounting. It is notable that underestimation and bluff-discounting did not occur in the same subject. However, this may reflect only the small sample size: There is no a priori reason why the two could not occur together.

One further aspect of the data deserves comment. In Experiment 2, nonmonotonicities occurred for small pairs, possible flushes, and possible straights. At the time, these seemed to reflect the same underlying cause. However, the data for Part 3 cast doubt on the equivalence of the three hand types in two ways. First, the bluff-discounting of Subjects 3 and 6 involves only possible flushes and possible straights. Second, all subjects show a nonmonotonicity for small pairs, including Subjects 1 and 5, who do not show it for either possible flushes or possible straights.

In retrospect, it seems that the nonmonotonicity for small pairs is caused differently than that for possible flushes and possible straights. A hand showing a small pair is ambiguous: It is better than average to begin with and potentially even better than that. It may be that when a moderately large bet-amount is given for a small pair, the subject attributes it to the pair already showing, rather than to the potential two-pair or three of a kind, and bets high because his 7s will beat the pair. Thus, when CALLED information is added, it has little effect, since the subject has already decided that the small pair showing is the reason for the bet.

Part 4: The integration of bet-amount and upcards. The results for Part 4 are given in the two panels of Figure 7. The open circles are the subjective likelihoods of beating the NO DATA hands listed on the abscissa, and the closed circles are the subjective likelihoods of beating the homologous standard hands. If the subjects combine information from bet-amount (12¢) and upcards (LLLH, LLHH, LHHH, and HHHH) by adding, the standard–NO DATA difference will be constant, independent of upcards. However, if averaging or multiplying occur, the differences will vary systematically with a crossover for averaging and a convergence toward high-threat upcards for adding.

The differences between homologous standard and NO DATA hands were computed separately for each subject for each replication and were submitted individually to analyses of variance. The results are given in Table 3. For Subjects 1 and 5, both statistical and graphical analyses support an averaging process: The characteristic crossover is clearly evident in the top panel of Figure 7. For the remaining five subjects, both statistical and graphical analyses support an adding process, as evidenced by the parallelism of the 12¢ and NO DATA curves in the bottom panel of Figure 7.

This apparent prevalence of an additive combination rule for bet-amount and upcards is notable, since adding processes have been difficult to demonstrate in natural judgment situations (Anderson, 1974a). While it is possible that adding occurs here because the two pieces of information do indeed bear on quite different aspects of beatableness, other experiments with seemingly different sorts of information have still shown averaging (e.g., Lampel & Anderson, 1968).

General Discussion

Modeling the Mathematical Structure of Poker

Assessing conditional likelihoods. The data from Part 2 of Experiment 3 indicate that experienced poker players are often qualitatively accurate in assessing the likelihood of beating particular sets of upcards: For five of seven subjects, the subjective likelihoods vary directly with objective likelihoods. However, quantitatively, there is an equally strong tendency toward underestimation. This qualitative–quantitative difference could reflect the use of a simple counting heuristic. If a player estimates the likelihood of beating a set of upcards by simply counting the number of its possible winning hole cards, he will be ordinally accurate. However, in so doing he may fail to attend sufficiently to the larger number of losing hole cards, and so may underestimate his own chances of winning.

Integrating likelihoods. The data from both Experiments 1 and 3 indicate that experienced poker players use an appropriate multiplicative procedure for integrating individual likelihoods into global likelihoods of winning, but the exact mechanism for this is as yet undetermined. One compelling possibility is serial fractionation. Suppose that a player begins each round with complete certainty of winning. After evaluating his first opponent's hand, this certainty is reduced by a factor proportionate to the opponent's likelihood of winning. Then, the player evaluates his second opponent's hand and reduces the remaining certainty again by a factor proportionate to the second opponent's likelihood of winning. This procedure is mathematically equivalent to multiplying the subjective likelihoods of beating the two opponents, but it is simpler, since it can be accomplished by direct analogical manipulation of subjective quantity and without the need for numerical computation.

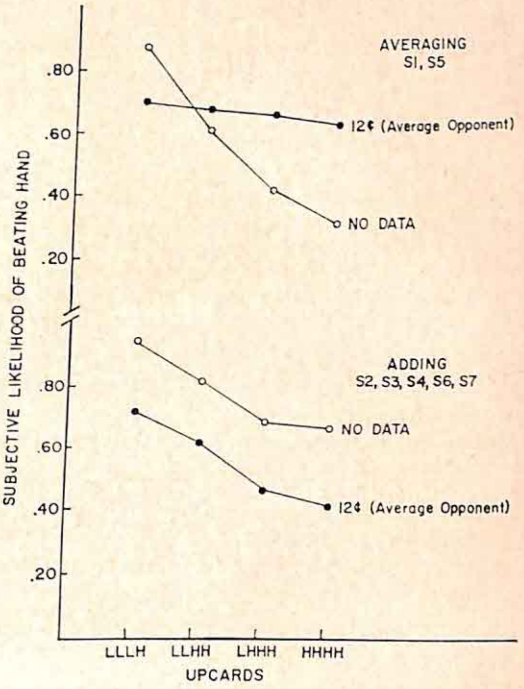

FIGURE 7. Data for Part 4 of Experiment 3: Averaging-versus-adding test for bet-amount and upcard information. (See Figure 1 caption for the definitions of the abbreviations used.)

Modeling the Psychological Nature of the Opponent

Scaling information. Experiment 2 revealed a multiplicative interaction between bet-amount and opponent-style. This suggests that players do not use playing style directly in their assessment of subjective likelihood, but rather they use it to scale bet-amount. Such a scalar process would be reasonable here, since it implicitly defines the style of an opponent as a variable that influences his monetary dealings, but not his luck in holding good cards.

Assessing credibility. In Part 3 of Experiment 3, two subjects reveal strong evidence of discounting bet information when the opponent is likely to be bluffing. Similar discounting mechanisms have been suggested previously as means for dealing with low source credibility (Anderson, 1971). However, in the present case, discounting is not a general mechanism applied to all bets from a particular opponent, but is reserved for instances where the opponent is especially likely to be bluffing. This is sophisticated and clearly represents a model-based process that permits the player to consider the potential threat of a hand from both defensive and offensive points of view.

Integrating evidence. Part 4 of Experiment 3 revealed sharp differences between subjects in rules for integrating bet-amount and upcards: Two subjects were averagers, and five were adders. While these rules have some formal similarity, they are quite different psychologically, and the use of one rather than the other may contribute to a player's perceived playing style. This may be seen most easily by considering how the two rules differ in their treatment of inconsistent and redundant information.

Both averaging and adding are sensitive to qualitative inconsistency: If two items of information differ in sign, the response under either rule will be less extreme than to either item alone. However, averaging is also sensitive to quantitative inconsistency and always yields a response that lies interior to the stimulus range. Adding is not as conservative in this respect: So long as the stimulus items are similar in sign, the response will be more extreme than to any item taken alone.

Similarly, averaging and adding differ in the conservativeness of their response to redundant information. Simple averaging processes do not respond to redundancy: The average of redundant information is identical to the information itself.[5] In adding, however, redundant information has potentially limitless influence: Unless ceiling effects or diminishing returns intervene, the addition of redundant information will continue to increase the extremity of a result.

Modeling Models

Some years ago, Simon (1957) observed that in order to predict a person's behavior in some particular situation, we must understand his model of the situation. Clearly this is the case with complex decision making. While it is useful at times to compare human decisions with optimal decisions derived from prescriptive models, this is not sufficient for discovering the processes that produce human decisions. Instead, the decision maker must be taken on his own terms and studied in a descriptive framework that can encompass his personal model of the world. (See, for example, Kahneman & Tversky, 1973; Tversky & Kahneman, 1973.)

The research presented here sheds some light on how models of the world function in the inference and decision making processes of experienced poker players. It also suggests that previous work in decision making may have underestimated man's capacity to cope with complex and uncertain environments. While it is undoubtedly true that people are severely limited in their ability to process information in unfamiliar environments, they are not equally hampered in environments for which they have developed adequate models. Under these conditions, people demonstrate great skill in interpreting, transforming, organizing, and even augmenting incoming information as necessary to produce stable and appropriate responses to complex stimuli.

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[5] Redundant information will produce set-size effects in averaging if there is an initial impression. However, in contrast to adding, the set-size effect will asymptote at the value of the redundant information and will never produce judgments that are more extreme than the components.

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Limulus Psychophysics: Spectral Sensitivity of the Ventral Eye

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SUMMARY

The ventral eye of *Limulus* contains only one type of photoreceptor. Behaviors mediated by the ventral eye provide an unambiguous representation of the function of that single-receptor type. Such behaviors can be compared with the results of acute, single-cell investigations to assay for the contributions of candidate neural codes in the regulation of behavior (cf. Uttal, 1973).

Using an unconditioned tail movement as the response, the psychophysical spectral sensitivity function mediated by the ventral eye of *Limulus* was measured. This psychophysical function peaked at 525 nm and showed evidence of strong absorption by the cuticle in the short-wavelength portion of the spectrum. Under the conditions of the present experiment, the threshold was 4.5 quanta absorbed per receptor per msec at 525 nm. The spectral transmission of the ventral eye cuticle was also measured. After correction for cuticle absorption, the psychophysical spectral sensitivity function was compared with previously reported spectral sensitivity functions obtained either from electrophysiologic (Millecchia, Bradbury, & Mauro, 1966; Nolte & Brown, 1970) or from microspectrophotometric (Murray, 1966) recordings from single, isolated ventral eye photoreceptor cells. All three functions exhibit a sensitivity peak near 525 nm; the corrected psychophysical and microspectrophotometric functions both display a second peak near 425 nm. A second experiment confirmed the reliability and validity of the 425-nm peak.

The coding implications of these findings were explored. A preliminary finding is that, in dichromatic or trichromatic visual systems, two-peaked receptor spectral sensitivity functions produce central, opponent response systems that are qualitatively the same as those produced by single-peaked receptors.

In most visual systems individual nerve cells are highly differentiated, and they are embedded in a complex network that influences their own activity. Even vertebrate cones have recently been found to be electrically coupled with one another over as much as five cone diameters (Baylor, Fuortes, & O'Bryan, 1971). Therefore, some investigators interested in fundamental aspects of photoreceptor function have sought preparations wherein such complex interactions were not present. The ventral eye of *Limulus* provides such a preparation, because there appears to be only one class of ventral eye photoreceptors, and they can be dissected out of the organism and studied in isolation. The use of such an acute, isolated preparation requires that one distinguish between cellular events that are merely *signs* of activity and events that are actually involved in *codes* analyzed by higher levels of the nervous system (cf. Uttal, 1973). The best test of the validity of such distinctions comes from studies of behaviors that are mediated by the neural structure in question. Activities that were only considered signs would become obvious codes if they were found to affect behavior and vice versa.

Studies of visually guided behaviors mediated by the ventral eye of *Limulus* therefore seem to be of interest. Little is known about the function of the ventral organ in relation to *Limulus* behavior: Patten (1893, pp. 27–29) reported that males exhibit components of mating behavior when the ventral organ is stimulated electrically. We have found that stimulating the ventral organ by light elicits an unconditioned, downward tail movement (Wasserman 1973b, 1973c; Wasserman & Patton, 1970). This tail movement has been used to study the dynamics of dark adaptation (Wasserman, 1975).

The present investigation addressed the problem of spectral sensitivity. There have been several studies of the spectral sensitivity of isolated photoreceptors from the ventral organ of *Limulus:* Millecchia, Bradbury, and Mauro (1966) made an initial study of the spectral sensitivity of the light-elicited receptor potential, as recorded by an intracellular microelectrode. Subsequently, Nolte and Brown (1970) made a more detailed investigation of that same problem. The results of the two studies using the microelectrode method generally agree with each other. In addition, studies of the photopigment(s) resident in single ventral eye receptors were carried out by Murray (1966). Using Mark's (1965) microspectrophotometer, Murray measured the difference spectrum of the resident photopigment(s).

These two different methods of determining the spectral sensitivity of single photoreceptor cells did not yield identical results: Both microelectrode investigations found that the spectral sensitivity could be well described by a single-peaked function with its maximum sensitivity near 525 nm, although the action spectrum was slightly narrower than that required by Dartnall's (1953) nomogram. However, the microspectrophotometric data exhibited two peaks: (a) one near 525 nm and (b) another peak near 425 nm, whose absorption was reduced to a value half that of the principal peak. If the best-fitting nomogram were used as a template, the absorption of the 425-nm peak was twice the value expected from a single-pigment system. This second peak was not considered important by Murray, although it was 5 standard errors above the nomogram value. The microspectrophotometric data also departed from the nomogram in the long-wavelength part, as well as in the short-wavelength part, of the spectrum. At 625 nm, the discrepancy was 10%, which was 1 1/2 standard errors. Thus, the microspectrophotometric results were slightly broader than predicted by Dartnall's nomogram.

Marks's (1965) analysis of such variations from the nomogram stressed the kinds of artifacts that can occur in microspectrophotometric investigations, although no specific artifact was identified as the source of the 425-nm peak by Murray (1966). Presumably, the 425-nm peak would have been taken as the sign of a photoproduct of the 525-nm photopigment formed during a spectral scan. Liebman's (1972) analysis of the technical and conceptual issues involved in microspectrophotometry describes a variety of other factors that can lead to random, as well as systematic, error. The 425-nm peak, therefore, has not been considered to represent a sensory characteristic that actually affects the behavior of the organism. It seemed appropriate then, to study the psychophysics of this problem; a microspectrophotometric artifact that affected behavior would merit reconsideration.

The question is a general one: Elsewhere (Wasserman, 1973a) I have summarized the evidence derived from a number of microelectrode investigations addressed to the problem of the spectral

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sensitivity of single, invertebrate photoreceptor cells. The majority finding of those studies leads to the conclusions that (a) some invertebrate photoreceptors (β cells) have spectral sensitivity functions that display two peaks of roughly equal sensitivity and that (b) artifactual explanations are implausible. Prior expectations were that all receptors would have spectral sensitivity functions that exhibited only one peak (α cells). Furthermore, the more recent application of digital computation techniques to the problem of vertebrate cone function suggests that goldfish cones are also β receptors and that the second peak is more variable than the principal peak (Harosi & MacNichol, 1974). In most of these investigations, the second peak of a β photoreceptor has not been considered as part of a genuine neural code. Artifacts have usually been postulated without demonstration. The questions deserved a behavioral test, and the ventral organ of *Limulus* provided an ideal preparation for comparing behavioral and single-cell results, because the ventral organ of *Limulus* contains only one homogeneous type of photoreceptor. Were there two or more receptor types, one would also require an accurate knowledge of their frequency and connectivity as well.

In order to compare isolated single-cell and behavioral results, it is necessary to take account of structural factors involved in the selective absorption of light by the cuticle that overlies the photoreceptors. Figure 1 illustrates the structural features of the ventral organ as well as its relation to the rest of the organism. The ventral surface of a female *Limulus* is shown in Figure 1A, while an enlargement of that portion around the ventral organ is shown in Figure 1B. The ventral organ is delineated by the roughly rectangular area between the two small feeding claws (chelicerae). On superficial examination, the ventral organ is usually seen as lighter and yellower than the surrounding cuticle; a darker bump, whose prominence varies, is found in the center of the organ.

Dissection of the ventral organ (Demoll, 1914; Hanström, 1926, 1928; Johansson, 1933; Patten, 1893) showed that it is innervated by two pairs of ventral nerves originating in the anterior brain, as in Figure 1C (which shows only that portion of the brain anterior to the oesophagus). The anatomy (as well as the physiology [Snodderly, 1971]) of this portion of the nervous system indicates that it is a visual processing area; nerves that originate in the large lateral compound eyes as well as in the median ocelli terminate here. (These other optic nerves are not shown in Figure 1.) The large lateral ventral nerves exhibit no significant branching. Single and multiple photoreceptor cells can be found anywhere along the length of the lateral ventral nerves; the largest number are at the distal end of the nerve. However, some photoreceptor cells are found at the proximal end of the nerve near the brain, while other cells are found at intermediate positions along the nerve (Clark, Millecchia, & Mauro, 1969; Fein & DeVoe, 1973; Millecchia & Mauro, 1969; Millecchia et al., 1966; Murray, 1966; Snodderly, 1971). Electron microscopy of the photoreceptor cells shows that they are morphologically similar to other invertebrate photoreceptors in that they have a prominent microvillous rhabdomere (Clark et al., 1969). The median ventral nerves are smaller and fused; they branch profusely and innervate chemoreceptors in the ventral organ cuticle, sparing the lighter zone over the distal end of the lateral nerve.

Figure 1D shows a coronal section through the ventral organ system at the level of the ventral organ. The cuticle is a layered system that varies from 300 μm to 1,500 μm in thickness. The outer cuticle is optically different from the inner cuticle. Examination of fixed sections showed that the outer layer fluoresces when illuminated with ultraviolet light; it can sometimes be seen to be frankly yellow under tungsten illumination. This yellow appearance was most noticeable at the ventral organ, while the fluorescence could be seen anywhere, even in the oesophageal lining. The chemoreceptors also fluoresce. The third optically significant layer is the dark pigment that underlies the cuticle. The

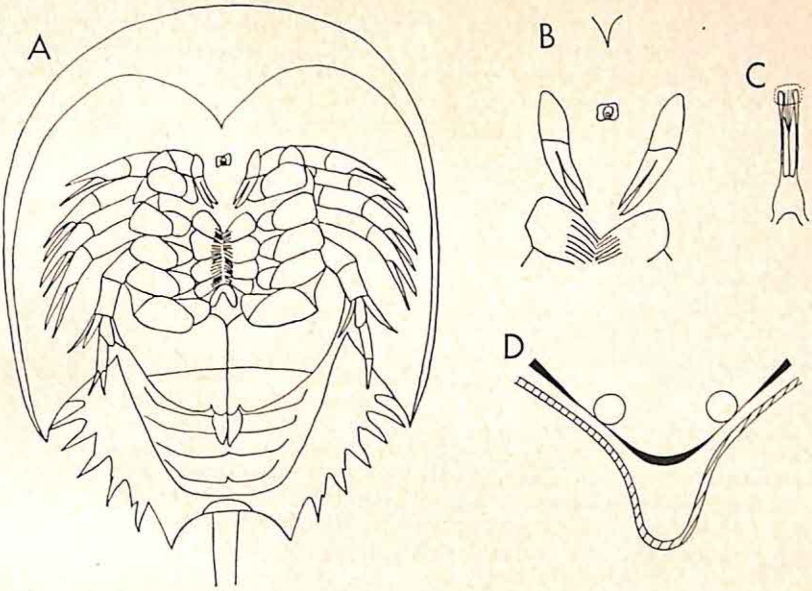

FIGURE 1. A. Ventral surface of a female *Limulus*. (This animal measured about 15 cm across the widest part of the carapace.) B. Enlargement of that portion of the ventral surface around the photosensitive ventral organ. (The roughly rectangular area between the small feeding claws (chelicerae) is the ventral organ.) C. Neural structures subjacent to the ventral surface shown to the same scale as in Figure 1B. (Dotted lines delineate the ventral organ. Also shown are the optic lobe of the anterior brain, the large lateral ventral nerves and the fused medial ventral nerves. Photoreceptors are located along the lateral ventral nerves and contribute axons to it.) D. Coronal section through the ventral organ. (The inner and outer cuticle layers are shown as clear and cross hatched, respectively. The dark pigment is shown as a black zone and the lateral ventral nerves are shown as circles. Chemoreceptors and fibers from the medial ventral nerves are not shown.)

amount of this pigment varies, and it apparently disappears within the lateral portion of the ventral organ, thereby producing the characteristic light appearance. The distal ends of the lateral ventral nerves are subjacent to the cuticle. The proximal portions of the lateral ventral nerves are more deeply situated. Superficial examination of the ventral surface of a variety of individual horseshoe crabs suggests that the extent and degree of pigmentation is highly variable.

The foregoing structural features lead directly to the correction required for the threshold case; one would ordinarily expect that threshold behavior would be mediated by those receptors most favorably situated with regard to the incident light. Such would be the case for those receptors located at the distal end of the lateral ventral nerves. Not only are these receptors not shielded by the dark pigment, but they are also covered by the thinnest cuticle as well as being subjacent to the cuticle. Accordingly, microspectrophotometry of the two layers of the cuticle and psychophysical measures of spectral sensitivity were both involved in the present study.

METHOD

The psychophysical technique used here was the same as that described in detail previously, unless otherwise specified (Wasserman, 1973b, 1975; Wasserman & Patton, 1970). Briefly, the animals were clamped into a harness that permitted only tail movements. A string was attached to the tail and to a sensitive potentiometer so that movements of the tail produced analog voltage signals. A gated analog detector produced a relay closure when the position of the tail changed by 6 mm from its position at light onset. A spring exerted an upward biasing force on the tail. This biasing force was adjusted at the beginning of each session by a servo mechanism, which sought an elevated set point. This permitted the emission of the unconditioned response to light, which is a downward tail movement. The servo mechanism was disabled during data collection. Spontaneous responses in the absence of light were measured during a foreperiod that immediately preceded the light.

Apparatus

The only substantial apparatus change was the substitution of an ultraviolet-transmitting fiber optic light guide (American Optical ULGM-3-48). This light guide was .35 cm in diameter and was placed under the ventral organ at a distance of 2–3 cm. The light emanating from the fiber optic guide diverged to illuminate a circular region approximately 1 cm in diameter and centered on the ventral organ. Zeiss (Jena) interference filters were used to select spectral stimuli, and they were individually calibrated in a Beckman DK-2 spectrophotometer. These filters were placed in a collimated portion of the light beam, and they contained integral dye filters that blocked any side bands. Six stimulus wavelengths, nominally spaced at 50-nm intervals from 375 nm to 625 nm, were used. The true peak wavelengths of these filters were 379, 427, 476, 524, 576, and 630 nm; they had half-peak bandwidths of 8, 11, 12, 4, 7, and 8 nm, respectively. Stimulus intensity was adjusted by a pair of counterbalanced neutral density Inconel wedges. Each stimulus wavelength was calibrated at every intensity setting by a Tektronix J16 digital radiometer equipped with a J6514 probe. The actual spectral calibration for this instrument was determined by Tektronix on two separate occasions, with virtually the same outcome.

Animals

As in previous work the animals used in this study were screened for the presence of the unconditioned tail response. The same screening criterion was used, namely, that after 3 days of testing the animals had to respond to a suprathreshold tungsten light at a 75% or better level. Of the 24 animals screened, 8 met the above criterion. Only 6 of the 8 completed one replication of the experiment, while 4 completed two replications, and 1 completed three replications. Therefore, the spectral sensitivity function is based upon 11 replications of the experiment in 6 animals. The spontaneous response level was 5.9%.

Procedure

Since individual animals can only be used for a limited number of sessions before they cease to respond, it is necessary to use an experimental design that monitors and controls for possible changes in responsiveness. The following design was used: Each session tested the response to one stimulus wavelength. The animal was dark adapted for 1 half hour before testing began. On the first trial, the neutral density wedges were set to their maximum attenuation. On each succeeding trial, a stepping motor increased the intensity by approximately .3 log units. Eleven trials, spaced at 120-sec intervals and covering an approximately 3.3-log-unit intensity range, were used in each session. The first session always used a 475-nm stimulus. The next 5 sessions involved the presentation of the other five stimulus wavelengths in random order. The final session involved a repetition of 475 nm. These 7 sessions constituted one replication of the experiment. If the performance on the final 475-nm check session indicated that the animal was no longer responding, the entire set of observations from that replication was discarded. The criterion for unresponsiveness was one response or less to the 6 highest intensities. In the 11 replications, the mean ratio of the responses in the second 475-nm session to that in the first session was 1.08, indicating a slight improvement in the responsiveness of the animals during the course of the replications.

Microspectrophotometry of the cuticle. A Zeiss dual beam microspectrophotometer (UMSP-1) was used for these measurements. Freshly removed blocks of tissue were sectioned in a cryostat without fixation; 40-μm sections were mounted on quartz slides in artificial sea water under quartz cover glasses. Identical but blank, slides were put in the microspectrophotometer reference beam. The 100% baseline was obtained from a clear area of the sample slide located 200 μm from the sample tissue. The clear area and the cuticle sample were alternately measured on successive scans through the spectrum. Repeated scans showed that the tissue did not change after excision. The actual transmission was calculated as the ratio of the nominal sample transmission to the nominal clear area transmission. The transmission was measured with either 13- or 20-μm spots.

It was not possible to measure the spectral absorption of the dark-pigment granules that underly the cuticle. No readings above the noise level could be obtained using 2-μm spots (which fall within the diameter of single-pigment granules). The transmission of the individual granules, therefore, can be no more than 1% at any point in the spectral range tested (350–650 nm). Scattering around the granules might increase the apparent transmission of the entire layer.

RESULTS

A representative set of microspectrophotometric scans of the outer cuticle are shown in Figure 2. For illustrative purposes, no filtering was employed. In most other scans, the high-frequency noise was electronically filtered. Table 1 gives mean transmission values for the outer and inner cuticle at the six stimulus wavelengths that are of interest. There is a reliable difference between the outer and inner portions of the cuticle, as would be expected from visual examination of fixed sections. However, both portions are demonstrably yellow and differ only in degree. Murray's (1966) absorption spectrum for the ventral photoreceptor cells is quite similar to the cuticular absorption spectra. Murray's spectrum peaked and declined at wavelengths much above 650 nm; in the few cases wherein data were obtained at such long wavelengths, a similar decline was observed for the cuticle. The transmission data from these 40-μm sections were converted into an overall absorption figure for the 300-μm section (thinnest part of the cuticle), taking 80 μm for the outer cuticle thickness and 220 μm for the inner cuticle.

The psychophysical data consist of a matrix of response frequencies stimulated by different combinations of wavelength and intensity. Threshold as a function of wavelength was extracted from these data by means of the same probit technique used previously to extract threshold as a function of time in the dark (Wasserman, 1975). As in that previous work, the response frequencies were converted to z scores in the probit domain. The same correction for the 80% ceiling effect was applied. Then, for each wavelength, the least squares best-fitting straight line was calculated as a function of log intensity. The zero intercept of the best-fitting line, corresponding to the 50% frequency, determined the threshold intensity. The scatter of the points about the lines in the present experiment was comparable to the scatter found earlier (Wasserman, 1975). The correlation coefficients for the actual data and the values predicted by the least squares probit functions were uniformly high, ranging from .74 to .95 with a mean of .88. The standard deviations of the thresholds, taken from the slopes of the probit functions, were somewhat higher in the present work; the mean standard deviation was 1.85 log units, as opposed to 1.5 log units found previously (Wasserman, 1975). This increase in the standard deviation probably reflects the difference in procedure: In the current work, higher intensities were confounded with time during a session. Any tendency for the animals to habituate or to become light adapted during a session would tend to reduce the response probability for the higher and therefore later intensities. The intensity-response functions would thereby have a lesser slope, increasing the standard deviation of the threshold. These elevated standard deviations again represent between-subjects, as well as within-subjects, variance.

TABLE 1

Mean Outer ($n = 18$) and Inner ($n = 21$) Cuticle Transmission (in percent) of 40-μm Sections at Six Stimulus Wavelengths (in nm)

Wavelength	Outer cuticle	Inner cuticle
379	41.3 (2.1)	60.0 (4.8)
427	58.4 (1.9)	62.5 (4.6)
476	67.1 (1.6)	65.7 (4.0)
524	74.6 (1.1)	68.7 (3.6)
576	77.7 (1.1)	70.2 (3.4)
630	80.2 (1.0)	73.7 (3.4)

Note. Standard errors are in parentheses.

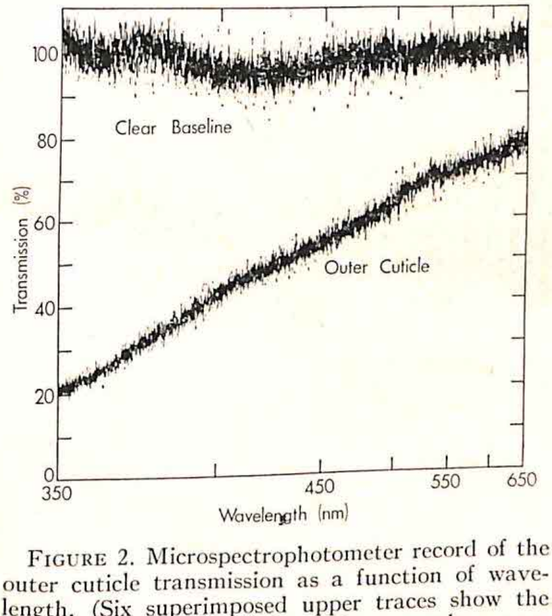

FIGURE 2. Microspectrophotometer record of the outer cuticle transmission as a function of wavelength. (Six superimposed upper traces show the 100% reference level taken from a clear area located 200 μm from the sample. Five superimposed lower traces show the transmission of the sample. Individual traces from the clear area and the sample area were alternated. Data are from a 40-μm section taken with 13-μm spots.)

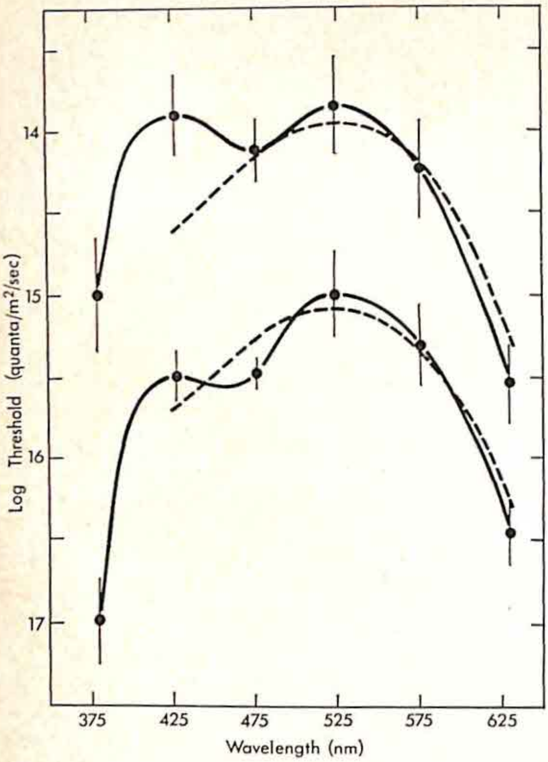

FIGURE 3. Quantum spectral sensitivity as a function of wavelength. (Lower solid curve is the uncorrected psychophysical function; upper solid curve has been corrected for 300 μm of cuticle absorption. Dashed lines are Dartnall's [1953] 525-nm nomogram fit to the four longest stimulus wavelengths. Vertical lines are ±1 standard error.)

The lower solid curve in Figure 3 shows the psychophysical spectral sensitivity function. The cuticle absorption correction was applied to those data, and the upper solid curve in Figure 3 is the corrected spectral sensitivity function. The corrected function provides a behavioral estimate of receptor spectral sensitivity. The standard errors of the corrected spectral sensitivity function are the vector sums of the standard errors of the cuticle correction function and the standard errors of the uncorrected psychophysical function. Both spectral sensitivity functions show a principal peak in the vicinity of 525 nm and an auxiliary peak in the vicinity of 425 nm. The 525-nm nomogram (Dartnall, 1953) is shown as a dotted line. Dartnall's nomogram was derived from the empirical finding that all photopigments in solution have difference spectra of the same form when plotted against the reciprocal of wavelengths. It therefore provides a reasonable guide to the light-catching properties of photopigments in situ, although there are usually small variations of in situ spectra from the nomogram. The vertical location of the nomogram was obtained by fitting it to the sensitivity values of the four longest stimulus wavelengths for both spectral sensitivity functions. The nomogram does not provide a very good fit to the uncorrected data; improving the fit at 425 nm by sliding the nomogram up would only worsen the fit at 475 nm. The corrected data clearly cannot even be approximated by the nomogram below 475 nm. However, in both cases, the nomogram provides a reasonable description of the long-wavelength portion of the spectrum.

A statistical examination of the finding of two peaks is relevant to the principal question of this study: An appropriate test of the null hypothesis that the auxiliary peak is a chance variation can be obtained by a one-tailed comparison of the experimental result at 425 nm with the result expected from Dartnall's 525-nm nomogram. In the case of the uncorrected data, the mean experimental result is 1.3 standard errors above the value predicted by the nomogram ($p < .10$). In the case of the corrected spectral sensitivity function, the mean experimental result is 2.8 standard errors above the value expected by the nomogram ($p < .01$). No matter which function is examined, the evidence suggests that there are two peaks in the spectral sensitivity function. After correction for cuticle absorption, the null hypothesis is rejected by conventional standards. Subsequent examination (see below) confirmed the reliability and validity of the 425-nm peak.

Dartnall's (1953) 525-nm nomogram does not extend down to 375 nm, although it is possible to extrapolate with some caution by assuming the usual leveling off at short wavelengths characteristic of photopigments in solution. The uncorrected sensitivity at 375 nm then can be seen to be about 4 standard errors below such an extrapolated nomogram. The corrected sensitivity function is still about half a standard error below the extrapolated nomogram at 375 nm. These considerations show that a very substantial cuticle effect occurs at the short end of the spectrum, and they suggest that our assumed 300-μm cuticle thickness may have been somewhat too low. However, for reasons that will be discussed below, it is preferable to undercorrect rather than overcorrect. Since the cuticle absorption spectra are linearly proportional to the reciprocal of the wavelength, it is possible that scattering is responsible for the cuticle's effect on light. Were scattering rather than absorption the major factor, then microspectrophotometry would not give a reliable estimate of the actual cuticle screen in situ, because the optical paths would be of grossly disparate lengths. However, the sensitivity values obtained at 375 nm demonstrate the cuticular effect in situ and indicate that such an artifact is not present.

The quantum sensitivity of this behavior at threshold can be estimated as follows. At the optimal wavelength (525 nm), threshold occurs at 1.0×10^{15} quanta/m^2/sec. Correcting for cuticle absorption reduces this figure to 8.5×10^{13} quanta/m^2/sec. According to Clark et al. (1969), the ventral photoreceptor cells are ellipsoids with a 60-μm minor axis and a 200-μm major axis. Since these receptors appear to be randomly orientated, the maximum cross-sectional area would be 9.4×10^{-9} m^2, the minimum would be 2.8×10^{-9} m^2, and the average receptor area can be taken as 7.0×10^{-9} m^2. This yields a receptor threshold flux of 6.0×10^5 quanta/sec. The rhabdomere of these photoreceptors meanders throughout the cell, and it is likely that the cross-sectional area of the receptor is no more than three times the cross-sectional area of the rhabdomere (photopigment); threshold per receptor then reduces to 2.0×10^5 quanta/sec. The visual pigment absorbs only 2.28% of the incident 525-nm quanta (Murray, 1966); at threshold each receptor then would absorb 4.5×10^3 quanta/sec. This represents a relatively low threshold quantum rate, namely, about 4.5 quanta absorbed in each receptor every millisecond.

Latency measures were also obtained in these experiments. As in earlier work (Wasserman, 1975), latency decreased with increasing intensity. Latency, therefore, might have provided an independent estimate of the spectral sensitivity function. However, the mean slope of the latency versus intensity function was close to 0 (−.11 sec/log unit), and random variation caused the slope to be negative for some wavelengths and positive for others. No unique spectral sensitivity function, therefore, could be extracted. The cause of the shallow latency versus intensity slope is probably the same as the cause of the increased standard deviation of the threshold, namely, the confounding of intensity with trial position within a session.

Reliability and Validity

Two questions might be raised about the results obtained in this investigation. First, the data come from a selected sample of animals. In this investigation, two thirds of the animals initially scanned were rejected because the response level was below criterion. Of the remainder, some were subsequently rejected because the response was unstable. Accordingly, it is possible that the data are unrepresentative of the population. Furthermore, the conclusion that a 425-nm sensitivity peak exists is only based on a single set of observations. The probability that this peak represents a random fluctuation is less than 1%; nevertheless, the implications of these results are such as to make it advisable to check the reliability of this finding as well as the validity.

Check experiment. A check experiment was designed to examine both the reliability and the validity of the finding. The logic of the check experiment involved using a large number of unselected animals and collecting only a small number of observations from each animal. Fluctuations in response potency, which were the reasons for selecting animals, would have a minor impact when only a few experimental

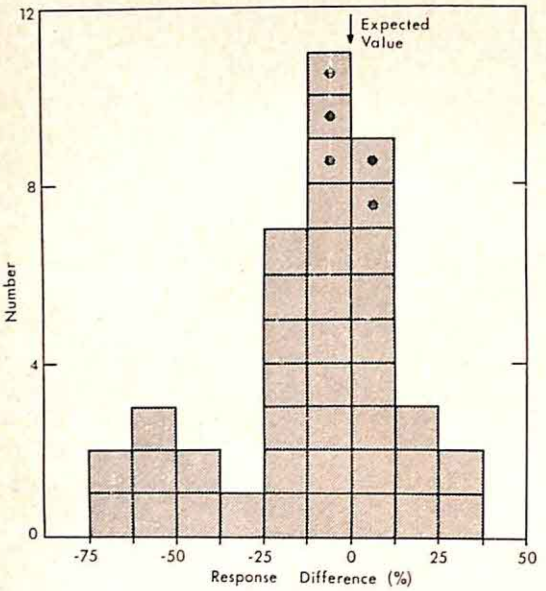

FIGURE 4. Histogram of response differences (in percent) between the response frequency elicited by threshold-level 425-nm light and the response frequency elicited by threshold-level 525-nm light. (Negative values indicate the 525-nm response frequency was greater than the 425-nm response frequency. The expected value is based on the assumption that the response difference equals 0. Solid dots indicate cases where the response frequency was below 10%.)

sessions were involved. The procedure involved exposing animals to threshold-level lights of either 425 nm or 525 nm; threshold was determined by the data of Experiment 1. The animals were exposed to the two lights in four sessions delivered in a counterbalanced ABBA design. The measurement of interest was the difference in the response produced during the two 425-nm sessions and the two 525-nm sessions. That difference would be relatively unaffected by irrelevant factors, provided that the animals were in fact responding. Twenty-two animals were tested a total of 40 times. The expected outcome of this experiment was that the response levels should average 0 with some random variation, if the finding of the previous experiment was both reliable and valid. Figure 4 presents a histogram of those response differences. Since a dead animal who never responded would give a response difference of 0, cases where the response difference was based on response frequencies below 10% are indicated with a solid dot.

Most of the observations cluster around the expected value of 0. Three of the 22 animals produced results that were substantially different from the expected value. These 3 animals hardly responded to the 425-nm stimulus while responding quite vigorously to the 525-nm stimulus; these results were repeatable in these animals. Apparently, the variability that has been encountered in measuring the spectral sensitivity of β cells with intracellularly placed microelectrodes (cf. Wasserman, 1973a) also exists when measuring behavioral responses from intact animals. However, the bulk of the data of this experiment confirm the findings of the previous experiment. Retrospective analysis of the data suggests that laboratory age was a major factor in producing responses that deivated from the expected value.

DISCUSSION

Had there been no prior single-cell measurements, the present psychophysical data would most likely have been interpreted in terms of two underlying receptor mechanisms, one with a 425-nm peak and the other with a 525-nm peak. However, neither the anatomical, nor the microspectrophotometric, nor the electrophysiological studies described above reported evidence of more than one receptor class, and so this hypothesis cannot be considered unless all three techniques have the same sampling bias, which is most unlikely.

A second possible explanation of these data would be that the cuticle correction (which enhances the 425-nm peak) is incorrect and excessive. However, the correction actually employed is the minimum possible and, if anything, errs on the low side. Any assumption that the cuticle is really thicker would only result in a greater elevation of the 425-nm peak relative to the 525-nm peak at the rate of 1 log unit/mm of thickness.

A third possible explanation of the data might be that cuticle fluorescence causes the 425-nm peak. Microspectrofluorometric measurements of the ventral organ cuticle are not available, but visual inspection of both the exciting light and the fluorescence in a fluorescence microscope make it very doubtful that the efficiency exceeds 1%, if it even approaches that level. In a fluorescence microscope, the tissue is excited by a powerful light that is filtered so that only ultraviolet and blue light reaches the slide. A barrier filter between the slide and the observer blocks the short-wavelength exciting light but permits any long-wavelength fluorescent emission to reach the observer. In the present case, the fluorescence appears green. A brief removal of the barrier filter therefore exposes the observer to the full short-wavelength exciting light, which light adapts the observer to the point where many seconds of dark adaptation are required to see the long-wavelength fluorescence after reinsertion of the barrier filter. Although this is an imprecise (as well as painful) experience, the dynamics of dark adaptation are such as to suggest that the fluorescent emission is orders of magnitude less than the incident light. Moreover, the observer's visual system is more sensitive to the long-wavelength fluorescence than to the short-wavelength excitation, which also suggests that the fluorescent energy is quite weak.

Now, the necessary fluorescent efficiency needed to account for the 425-nm peak can be calculated from the following considerations: The most favorable case would be if all of the reemitted 425-nm quanta come out as 525-nm quanta (at the peak of the electrophysiologically determined spectral sensitivity curve). Such quanta would be 5.4 times more effective than 425-nm quanta in exciting the photoreceptor. This increase in effectiveness due to fluorescence would have to account for the .71-log-unit increase in sensitivity at 425 nm shown in Figure 3. Since .71 log units represents a 5.1-fold increase in sensitivity, fluorescence must be able to account for this increased sensitivity. The following expression can be formulated to describe this outcome:

$$Q_{425} + 5.4Q_{525} = 5.1, \qquad (1)$$

where Q_{425} represents the number of 425-nm quanta, and Q_{525} represents the number of 525-nm quanta. But $Q_{425} + Q_{525} = 1$, by the definition of fluorescence, and so Equation 1 is satisfied when $Q_{425} = .07$ and $Q_{525} = .93$. Thus on the most favorable possible assumption, the efficiency of the fluorescence would have to be 93% in order to account for the data. Measurements by Adolph (1968) of fluorescence in the *Limulus* lateral eye cornea show that the emission peak is not at 525 nm but rather is at 450 nm. Visual inspection shows no striking differences in the fluorescence of the ventral eye cuticle and the lateral eye cornea. So, if we assume that the fluorescent quanta are actually reemitted at 450 nm, those quanta are 1.6 times more effective than 425-nm quanta in exciting the receptor. From an analysis similar to the one given above, we arrive at the impossible outcome that the fluorescent efficiency would have to exceed 100%—it would in fact have to be 683%. These results would be modified somewhat if we knew how to account for the wavelength-dependent absorption of light by the cuticle. However, such effects are difficult to assess, since the fluorescence occurs within the cuticle. In any event, such an evaluation would not materially affect the conclusion given above, namely, that on the best assumptions, the fluorescent efficiency would have to approach 100% in order to conclude that the 425-nm peak was a harmonic produced by fluorescent coupling to the true peak. That degree of efficiency is most unlikely.

It therefore becomes appropriate to compare the psychophysical measurements of spectral sensitivity in the ventral eye of *Limulus* with electrophysiological and microspectrophotometric determinations of the same function. Figure 5 shows all three functions together; the corrected psychophysical function is illustrated. The low sensitivity found at 375 nm reinforces the notion that the cuticle correction was conservative. The three spectral sensitivity functions form a continuum: The receptor potential function shows no sign of two peaks, the microspectrophotometric func-

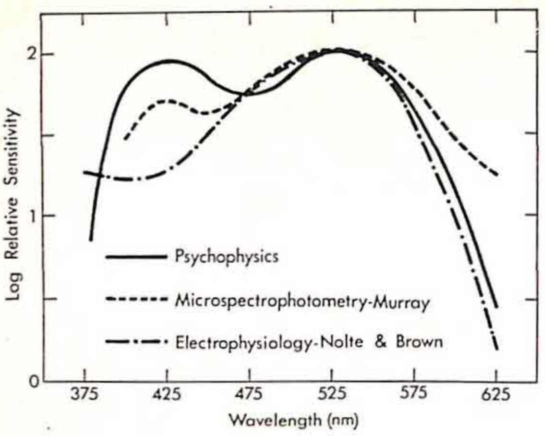

FIGURE 5. Psychophysical, microspectrophotometric (Murray, 1966), and electrophysiologic (Nolte & Brown, 1970) spectral sensitivity functions equated at 525 nm. (The psychophysical function has been corrected for cuticle absorption.)

tion shows two peaks with the auxiliary peak attenuated by .3 log units, while the behavioral function shows two almost equal peaks. An unavoidable property of microspectrophotometric data is that photoproducts can affect a difference spectrum. Thus the relative height of the two microspectrophotometric peaks in Murray's (1966) work could have been so affected. It has often been noted that photoproducts could affect prebleach data, so as artifactually to produce a second peak. The obverse is equally possible in that postbleach photoproducts could reduce the height of a genuine second peak. Therefore, the relative height of the two peaks in the microspectrophotometric spectrum is not particularly informative, per se. All three functions are similar throughout the long end of the spectrum except for the extreme long-wavelength end; the microspectrophotometric function is broader than would be expected from the nomogram, while the receptor potential and psychophysical functions are both somewhat narrower. The widening of this end of the microspectrophotometric function looks substantial in these logarithmic units; however, the deviation was only 1 1/2 standard errors in the original data, and it is conceivable that it was a random variation.

While the findings of the present study are obviously subject to critical examination and reinvestigation, several tentative conclusions can be stated. First, a two-peaked description of ventral eye receptor spectral sensitivity is appropriate. Second, Murray's microspectrophotometric findings appear to be correct. Third, intracellular recording methods were unable to sense the presence of the second peak. Fourth, the acuity theory of β cells advanced earlier (Wasserman, 1973a) is probably invalid, since the ventral eye of *Limulus* can, at best, only distinguish left from right.

Coding Implications

As a result of these findings, it became increasingly difficult not to begin a preliminary consideration of the coding properties of visual systems with β receptors, given that the elements of such a code have been found to influence behavior. An earlier examination of this same question has been given by Burkhardt (1964). At least three possible visual systems can now be conceived: (a) all of the receptors might be α receptors, (b) all of the receptors might be β receptors, or (c) some of the receptors might be α and others might be β. Figure 6 shows these three possible outcomes for the simplest possible color vision system, namely, a dichromatic system that contains two kinds of photopigments, two kinds of photoreceptors, and two kinds of opponent-process central detectors. The left column shows the hypothetical photoreceptor spectral sensitivity functions that might exist; the assumed photopigments are Dartnall (1953) pigments. One pigment has its peak sensitivity at 475 nm, and the other has its peak sensitivity at 625 nm. Other pigments might have been chosen with other spectral spacings; the results would remain essentially the same.

Each of the pigments might be well segregated within a single photoreceptor. The spectral sensitivity of the photoreceptor would then entirely reflect the spectral sensitivity of the resident photopigment. However, it is also possible that

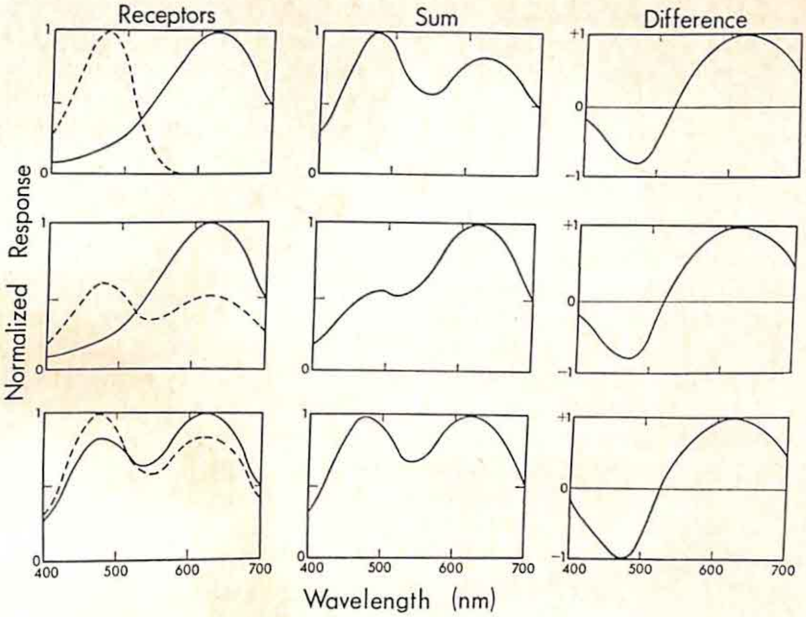

FIGURE 6. Normalized receptor and central detector spectral sensitivity functions. (Upper row is for the dichromatic visual system with only α receptors, the middle row is for both α and β receptors, and the bottom row is for only β receptors. The spectra in the sum column were obtained by adding the values for the two receptors and normalizing the highest value to 1.0. The spectra for the difference column were obtained by subtracting the values for the two receptors and normalizing the highest to ±1.0. The receptors were constructed from Dartnall's [1953] 425-nm and 525-nm nomograms.)

a photoreceptor contains some of each pigment. In that case, the spectral sensitivity of the photoreceptor would reflect the relative contributions of the resident photopigments. The middle row of Figure 6 shows a case where one receptor contains equal amounts of the two pigments, while the other contains only one pigment. Finally, it is possible that both of the receptors contain both pigments but in slightly different degrees. In that case (illustrated in the bottom row of Figure 6), both receptors would be β receptors, and the resident photopigments would produce photoreceptor spectral sensitivities that would reflect the difference in the amounts of the two pigments.

The central analyses of photoreceptor signals are known to be determined by neural circuits that take the sum and difference of receptor activities in the manner prescribed by opponent response theories of color vision (cf. Hurvich & Jameson, 1957). The sum yields activity that is considered to be related to the brightness or luminosity of the stimuli, while the difference yields activity that is considered to be related to the color or chromaticity of the stimuli. Accordingly, Figure 6 shows sums and differences for each of the three hypothetical receptor situations. The sums are the simple sums with no weighting coefficients. After summation, the data were normalized so that the highest value equaled 1.0. The same operations applied to the differences. When the two entirely α receptors are considered, the sum produces a luminosity function with two clear peaks. These peaks are a consequence of the wide spacing of the two photoreceptor spectra. Had we assumed a trichromatic system with three photoreceptors (including one in the middle of the spectrum), the sum would have produced a single-peaked luminosity function. This function would have been similar to the luminosity function obtained from trichromatic organisms, such as man. However, it is much simpler to illustrate these matters using a dichromatic system; a trichromatic system would have required us to explore eight possible visual systems instead of three. The difference between these two receptors produces an opponent response function that is quite familiar and representative. However, the entirely β receptor system produces sums and differences that are only quantitatively distinguishable from the sums and differences produced by an entirely α system. These quantitative differences could have been completely eliminated by quantitative adjustments in the relative numbers of photoreceptors impinging on the sum and difference detectors, as well as by quantitative differences in the synaptic efficiencies of the connections between the receptors and the sum and difference detectors.

Thus, a quantitative change in the degree of segregation of photoreceptor pigments produces a quantitative change in the sensitivity functions of central feature detectors. These central changes could have been entirely eliminated by other quantitative changes in the efficacy of the connections between the receptors and the central detectors (i.e., by changing the coefficients in the transfer equations). The implication of this analysis is that recordings of central feature detector spectra probably cannot discriminate among the three hypothetically possible configurations of the photoreceptors. Nor would other, more refined investigations be expected to be sensitive to these rather striking receptor changes. For example, wavelength discrimination would be expected to be approximately the same in all three cases: The neutral point of all three difference spectra occurs at about the same point in the spectrum, and the spectra have about the same slope as they go through the neutral point, implying that all of these visual systems would lead to high discriminability in the same place in the spectrum. However, a refined analysis of wavelength discrimination might be expected to reveal subtle consequences of variations in receptor spectral sensitivity functions. Helmholtz's (1892) line element analysis of wavelength discrimination data leads to the conclusion that each of the three component mechanisms of color vision should have a spectral sensitivity function with two peaks. A fresh reconsideration of this problem might well be productive.

A preliminary analysis of the coding properties of β receptor systems then indicates that no substantial effects are to be expected with regard to such fundamental phenomena as spectral sensitivity and wavelength discrimination, although subtle changes should be considered. While that outcome undoubtedly has contributed to the belated recognition of the existence of β receptors, it undoubtedly calls for a far more detailed investigation of the quantitative aspects of coding in visual systems that make use of β receptors. Perhaps such an analysis might lead to an understanding of the evolutionary advantages that have selected for the elaboration of β photoreceptors by such a large variety of organisms.

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ERRATUM TO FRIEDMAN AND BOURNE

In the article "Encoding the Levels of Information in Pictures and Words" by Alinda Friedman and Lyle E. Bourne, Jr. (*Journal of Experimental Psychology: General*, 1976, Vol. 105, No. 2, pp. 169–190), two of the entries in Table 1 on page 176 were incorrect. The mean reaction time of the ++ condition with a geometric solution should read 539.73, and the mean reaction time for the +− condition with a geometric solution should read 870.04.

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A Fuzzy Set Approach to Modifiers and Vagueness in Natural Language

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SUMMARY

Recent developments in semantic theory, such as the work of Labov (1973) and Lakoff (1973), have brought into question the assumption that meanings are precise. It has been proposed that the meanings of all terms are to a lesser or greater degree vague, such that, the boundary of the application of a term is never a point but a region where the term gradually moves from being applicable to nonapplicable.

Developments in fuzzy set theory have made it possible to offer a formal treatment of vagueness of natural language concepts. In this article, the proposition that natural language concepts are represented as fuzzy sets of meaning components and that language operators—adverbs, negative markers, and adjectives—can be considered as operators on fuzzy sets was assessed empirically. In a series of experiments, we explored the application of fuzzy set theory to the meaning of phrases such as *very small, sort of large*, and so on.

In Experiment 1, subjects judged the applicability of the set of phrases to a set of squares of varying size. The results indicated that the group interpretation of the phrases can be characterized within the framework of fuzzy set theory. Similar results were obtained in Experiment 2, where each subject's responses were analyzed individually. Although the responses of the subjects, in general, could be interpreted in terms of fuzzy logical operations, one subject responded in a more idiomatic style.

Experiments 3 and 4 were attempts to influence the logical–idiomatic distinction in interpretation by (a) varying the presentation mode of the phrases and by (b) giving subjects only a single phrase to judge.

Overall, the results were consistent with the hypothesis that natural language concepts and operators can be described more completely and more precisely using the framework of fuzzy set theory.

Picture a conversation between a mother and her 5-year-old son. The son asks the mother if he might have some jelly beans from the bowl on the table. The mother replies that he may take a few, and he does just that. It is a perfectly normal conversation until one stops and considers how many *a few* are. The mother obviously knows what is meant by the term *a few*. The information was understood by her son because he responded correctly to the mother's statement. If the son had been asked directly how many constitute *a few*, he probably would have hesitated for a moment and then responded "three" (or "four," or "five," and so on). Pose the same question to the mother, and the odds are that she would have responded with not too different a number. The number of beans taken might have differed from what either person might have replied, but the actual number was probably an acceptable definition from the mother's point of view. If it were not, the son certainly would have heard about it!

This scene demonstrates the transmission of some vague, quantitative information between two people. But actually, when one stops and considers, most of the quantitative information that one receives during the course of a day is of just this nature. A house may be *quite large*; a girl may be *sort of short*; a man may be *not very old*. Not only do people understand such statements, they also have the ability to operate upon and manipulate these vague concepts.

Recently, there has been considerable interest on the part of linguists and computer scientists in just such problems as the role of vagueness in language and the quantification of meaning. Much of this interest has been the result of the development of fuzzy set theory, a generalization of the traditional theory of sets. A major feature of fuzzy set theory is that a quantitatively specifiable system can contain linguistic variables in addition to numeric variables. These linguistic variables can be manipulated and operated upon in much the same way as numeric variables in nonfuzzy systems.

This new way of dealing with complex systems appears quite promising in terms of the specification of complex behavioral processes, such as the measurement of word meaning or the description of reasoning processes in everyday situations. In this article, we explore the possibilities of developing a treatment of (a) natural language concepts as fuzzy sets and (b) modifiers as operators on fuzzy sets.

Quantification of Meaning

One of the first attempts at the quantification of meaning was made by Mosier (1941). Mosier hypothesized that the meaning of a word may be considered as containing two components: (a) a constant component reflecting the overall meaning value along a continuum and (b) a variable component representing the variation in the meaning of the word due to context, speaker, and the like. He defined the meaning (M) of a word as:

$$M = x + i + c,$$

where x equals the constant component over people and context, i equals the variation in meaning due to the individual, and c equals the variation in meaning due to the context.

According to this theory of word meaning, any one of the components could be zero for certain words, and for ambiguous words, x could have multiple values. In addition, there is an assumption of a unidimensional continuum along which every word must fall. The model also predicts, for example, that context effects will be independent of the word used and the individual involved. Although these and other simplifications reduce the model's explanatory and predictive power considerably, it was probably the first significant attempt made to quantify meaning.

More interesting than Mosier's (1941) theoretical formulations are his empirical investigations in the same article. He had subjects rate a list of evaluative adjectives (e.g., unsatisfactory, excellent) along a favorable–neutral–unfavorable continuum. He then scaled the responses by the method of successive intervals. The scale values for each word were interpreted as the constant component of the meaning of the word, while the spread of the distribution (i.e., the vagueness) was interpreted as ambiguity, or the variable component. The data tended to support his model in that it was possible to assign each word to a scale value along a unidimensional continuum, and the variation in meaning about this scale value was normally dis-

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tributed. (These results were later supported by Jones and Thurstone, 1955, for a set of preference words and phrases.)

Mosier's subjects also rated a subset of the adjectives paired with adverbial modifiers, or intensifiers (e.g., very, extremely). An analysis of these data showed that the addition of an intensifier caused a shift in the *meaning* of the base word away from the neutral point toward the extreme. This finding that adverbs tended to cause a shift in the scale values served as the basis for a later study on the influence of adverbs by Cliff (1959).

Mosier had shown that the meaning of an evaluative adjective could be represented as a point along a unidimensional continuum reflecting favorableness. Osgood and his colleagues (Osgood, 1952; Osgood, Suci, & Tannebaum, 1957) felt that it was possible to specify the meaning of a wider range of words by rating the words on a judiciously chosen set of scales. These scales were combined to form the semantic differential. Using this procedure, a subject is presented with a word (e.g., quicksand), which he characterizes by rating it along a number of antonymous scales, such as clean–dirty, fast–slow, and serious–humorous. The resulting profile is considered to be a multidimensional representation of the meaning of the word. Osgood's (1957) factor analysis of the various scales of the semantic differential yielded three main factors, which could be called *potency*, *activity*, and *evaluation*.

According to Osgood (1957), the similarity between two words (or between the same word for different populations) is interpreted as the Euclidean distance between corresponding profiles. Using this definition of the distance between concepts, Rowan (Note 1) had subjects rate a list of words (e.g., sleep, hero, gentleness) using the semantic differential and the method of triads. The distances between word pairs obtained from a multidimensional scaling of the triad data correlated highly with the distances from the semantic differential. In addition, the first two dimensions from the multidimensional scaling could be interpreted as evaluation and potency/activity, the major factors derived from the semantic differential.

In the Mosier (1941) and the Jones and Thurstone (1955) studies, the rating scale (unfavorable–neutral–favorable) could be considered to capture a dimension of the meaning of the terms used. Thus, the words were rated according to the relative value of the corresponding denotative, or extensive, component. These ratings can be interpreted as the meaning of the words to the extent that both share a similar quantitative interpretation in the same context. Similarly, the semantic differential seems to demonstrate that subjects have the ability to rate the words in such a manner that overall differences between words are reflected in differential scale values. The semantic differential, however, does not demonstrate that the scale values reflect in any way the overall meaning of a word. Subjects may have the ability to rate a word such as *moon* as having a value on a *kind–cruel* scale. Even if this rating reflects a connotative property of the word, connotation is but one aspect of the meaning of the word. However, most scales on the semantic differential appear to reflect such connotative components. To suggest that word meaning can be uniquely specified by any number of such (nonorthogonal) scales implies that these scales represent all possible qualitative components in meaning—connotative and denotative. It has not been demonstrated that the semantic differential represents the full meaning of words in any comprehensive manner. Whether such a theory could ever adequately represent meaning is unclear.

The above studies have all represented meaning as a point along one or more rating scales. Mosier (1941) showed that the meaning of evaluative adjectives could be represented by scale values. How then might adverbs be characterized? Cliff (1959), extending Mosier's findings, proposed a formal model where adverbs functioned as multiplicative constants. If the scale value of the jth adjective is s_j, then the value of an adverb–adjective combination is represented as:

$$x_{ij} = c_i s_j + K,$$

where x_{ij} equals the obtained scale value of the ith adverb in combination with the jth adjective, c_i equals the multiplying value of the ith adverb, s_j, equals the psychological scale position of the jth adjective, and K equals the difference between the arbitrary zero point of the obtained scale values and the psychological zero point of the scale.

To test the model, Cliff had subjects rate a set of adjective–adverb combinations (e.g., extremely lovable) along a favorable–unfavorable continuum, as in the Mosier study. Over all combinations of 10 adverbs and 15 adjectives, the predictions of the model were extremely accurate. Values of c_i and s_j were completely independent. The only deviation from the model was that the value of K appeared to vary slightly as a function of the adjective employed. Overall, the hypothesis that adverbs operate as multipliers was clearly confirmed. The results explain why combinations such as *unusually average* appear so awkward: The scale value of *average* is approximately zero, so multiplying zero by any finite value will leave a product of zero—the adverb is superfluous.

Quantification of Vagueness

An underlying assumption in the work on meaning quantification has been that the meanings of words can be specified as points along a scale. The variability about the scale value is attributable to the statistical nature of the system. Recent developments in semantic theory, such as the work of Labov (1973) and Lakoff (1973), have brought into question the assumption that meanings are precise. Instead, it has been proposed that natural language terms are to a lesser or greater degree inherently vague, such that, the boundary of a term is never a point but a region where the term gradually moves from being applicable to nonapplicable. Though this question of the inherent vagueness of language has only recently become of concern to psychologists, it has occupied the attention of philosophers since the time of the Greeks, who posed the question of vagueness in the form of the paradoxes of *sōrites* (the heap) and *falakros* (the bald man). The latter paradox might take the form: How many hairs must be plucked from a man's head before he is considered to be bald? And given that there occurs a transition from being not bald to being bald, where is the one strand of hair that determines the transition, that is, where does one draw the line?

This paradox raises obvious difficulties for the law of the excluded middle: It appears unreasonable to assume that for a concept such as baldness every element of the universe is either a clear member of the set or a clear member of the complement of the set. The problem does not exist for artificial concepts with sharply defined boundaries, but appears unsolvable for vague, natural language concepts.

But what is a vague concept, and can it be specified? This was a rather productive area of research in philosophy during the early part of the twentieth century (e.g., Black, 1937; Copilowish, 1939; Hempel, 1939; Peirce, 1902; Russell, 1923; see also Black, 1963; Korner, 1957; Labov, 1973; Schmidt, 1974). Russell (1923) argued that the world is neither vague nor precise: It is what it is. Vagueness (and its complement, precision) are characteristics that can only belong to a symbolic representation, such as language. He further argued that a representation is vague when there exists a one-to-many relationship between the representing system and the system being represented.

Black (1937) agreed that the problem of vagueness is a property of natural language, but he faulted Russell for confounding vagueness and generality (p. 432, Note 12). Black differentiated these two terms from a third, ambiguity. Generality is what Russell described as vagueness, that is, a one-to-many relationship between a symbol and the items that the symbol represents. Ambiguity is the state of affairs where the same phonetic form has a finite number of alternative meanings. The vagueness of a symbol, however, will not be found as the result of a one-to-many relation nor a number of alternative meanings. Black reasoned that vagueness is a feature of the

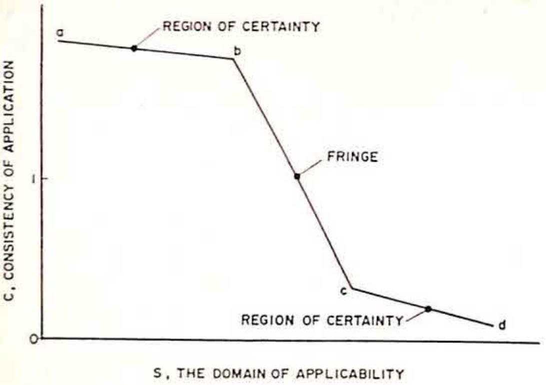

FIGURE 1. Hypothetical consistency profile.

boundary of a symbol's extension, not of the symbol itself. No matter how closely one looks or accurately one measures, the vagueness remains.

Peirce (1902) defined vagueness in a similar manner: "A proposition is vague when there are possible states of things concerning which it is intrinsically uncertain whether, had they been contemplated by the speaker, he would have regarded them as excluded or allowed by the proposition" (p. 748). There are some objects that a group of speakers of a language would definitely consider to be *chairs*; others that would never be called *chairs*. However, there will always be some objects that tend to straddle the boundary; no matter how closely one examines them, these objects cannot be classified as clearly belonging or not belonging to the category in question. It appears then that vagueness enters in the process of mapping a linguistic term onto a universe. That is, what is vague is the use of the linguistic term. (The term *natural language concept* thus refers to the result of this mapping operation, while *linguistic term* refers to the verbal label applied to the particular mapping. However, throughout the present article these two labels are used interchangeably to refer to both a term and its application.)

Black (1937), using what he termed a "consistency profile," first attempted a quantitative description of vagueness as defined by Peirce (1902). He hypothesized that while the vagueness of a word implies variability in the application of a term by a group of language users, the variations should be specifiable and systematic. If they are not, it would be impossible to distinguish between terms. He defined the consistency of application of a term, T, to an element, s, of a set, S (i.e., $S = \{s\}$), as in:

$$C(T, s) = \lim_{\substack{M \to \infty \\ N \to \infty}} \frac{M}{N},$$

where M equals the number of judgments that T applies to s, and N equals the number of judgments that *not* T applies to s. The consistency profile was then defined as the function $C(T, s)$ over the domain of applicability, S. Figure 1 depicts a typical example, where it can be seen that the most doubtful cases correspond to $C(T, s) = 1.0$. This profile, then, was used to define the vagueness of a term by taking the slope of the curve from Point b to Point c (see Figure 1) as an index of vagueness.

In the same article, Black put forth alternative formulations for describing the vagueness of a term. He redefined the consistency of application as $T(s, C)$. Using this latter notation, a term, T, will be said to apply to an item, s, of a series, S, with a consistency, C. The term *not* T will then apply with a consistency, $1/C$. Thus, the law of the excluded middle can be replaced by a reciprocal relation where the product of the applicability of a term and its complement is always unity.

Attacking the same problem of vagueness in language, Hempel (1939) redefined the consistency of application of a term, T, to an object, s, as:

$$C(T, s) = \lim_{\substack{M \to \infty \\ N \to \infty}} \frac{M}{M + N},$$

where M and N are as defined above. As a consequence, the range of C is restricted to the closed interval 0 to 1, and the "doubtful" cases take on values of about 1/2. (In a recent study of word boundaries, Labov, 1973, implicitly used just this type of formulation to describe the manner in which the form of objects and context interact to influence the extension of word boundaries.)

Hempel argued that Black's technique of specifying the vagueness of a term as the slope of the borderline region was inadequate, since the scale on the abscissa was arbitrary. Hempel reasoned, using his formulation, that the greater the vagueness in a term, the more objects there would be for which $C(T, s)$ takes on values around 1/2. The precision (pr) of a term then could be formalized as:

$$pr = \frac{4}{N}\sum_{k=1}^{N}[C(T, s_k) - \tfrac{1}{2}]^2,$$

and the vagueness (vg) as:

$$vg = 1 - pr.$$

Specifying the vagueness of a term in this manner, independent of the slope of the consistency profile, avoids another problem encountered by Black (1937). As Figure 1 shows, there are two regions of certainty ($a - b$ and $c - d$) and a region connecting these, which has been called the borderline region, or fringe. Black specified the vagueness of a term as the slope of the fringe. But what delimits the fringe area? Are there precise points at which the uncertainty begins or ends (as depicted in Figure 1)? One seems drawn to the conclusion that the extent of the boundaries of a term's application seems to fade in and out in an almost imperceptible manner. There is a smooth, continuous transition from applicability to uncertainty to nonapplicability. The entire extension of the term must then be considered to be vague, for the boundary extends (asymptotically) along the entire continuum.

The above discussion of the presence of vagueness in natural language argues against the Thurstonian model of meaning as put forward by Mosier (1941) and Cliff (1959). Such a theory implies that natural language concepts are precise and thus can be represented as points along a continuum. The observation that concepts form distributions is explained in terms of the statistical nature of the system. But if one accepts the theoretical position that natural language concepts are inherently vague, then the point concept becomes an inappropriate theoretical construct. Not only can a vague concept refer to a range, but also the variability is an integral part of the meaning of the concept.

Theory of Fuzzy Sets

Computer scientists and systems engineers have long recognized that people can understand and operate upon vague, natural language concepts. Computers, however, are extremely rigid and precise information-processing systems. This inherent rigidity severely limits a computer's ability to abstract and generalize fundamental conceptual functions. Recently, Zadeh (1965, 1973) and others (e.g., Goguen, 1967, 1969; Santos, 1970; LeFaivre, Note 2) have developed quantitative techniques for dealing with vagueness in complex systems. The techniques are based on fuzzy set theory, a generalization of the traditional theory of sets.

The unique feature of fuzzy logic is that it allows complex systems to contain both numeric and linguistic variables, where a linguistic variable is defined as a label of a fuzzy set. For example, the linguistic variable *age* may take on values of *very young, rather young, young, middle age, not very old*, and so on, in addition to a range of numeric values. The assumption underlying these fuzzy sets is that the transition from membership to nonmembership is seldom a step function; rather, there is a gradual but specifiable change from membership to nonmembership. In nonfuzzy set theory a membership (characteristic) function specifies which elements are members of the set (i.e., for which elements $x \in X$ has a truth value of 1). In fuzzy systems, the grade of membership and the corresponding truth value of the proposition $x \in X$ may take on any value in the closed real interval from .0 to 1.0.

Fuzziness is distinctly different from uncertainty as measured by the probability of an event. The uncertainty of a coin toss resulting in a head has a certain probability associated with it. No vagueness is involved—only lack of knowledge concerning an event occurring in the future. Once this knowledge becomes available, the state of affairs is completely determined. Referring back to the problem of *falakros* (the bald man), the concept *baldness* can be considered to be fuzzy. Unlike a coin toss, no matter how closely one measures or examines, the concept will apply more to some elements of the universe (men) than to others. No amount of information can make the boundary between *bald* and *not bald* free of imprecision.

To aid in discussions later in this article, it might be helpful at this point to summarize some of the important properties of fuzzy sets and show how traditional set theory is a special case of fuzzy set theory. (Much of this discussion is taken from Zadeh, 1965 and 1973.) Let X be a universe of points (or elements, or objects, or . . .) with a generic element of X denoted by x. A fuzzy subset of X, labeled A, is characterized by a membership function, f_A, that associates with each element x in X a real number, $f_A(x)$, in the closed interval from .0 to 1.0, which represents the grade of membership of x in A. An element of the fuzzy set A thus can be designated by the ordered pair:

$$[f_A(x), x], \qquad (1)$$

where $f_A(x)$ is the grade of membership of x in A. Note that the nearer the value of $f_A(x)$ to 1.0, the higher the grade of membership of x in A. When A is a nonfuzzy set, its membership function can take on only values of 1 and 0 according to whether x does or does not belong to A, respectively.

As an example, consider the concept *tall*, where the membership function specifies the grade of membership of heights (in inches) in the set labeled *tall*.[1] Representative values might be: $f(60) = .0$, $f(66) = .2$, $f(68) = .7$, and $f(74) = 1.0$. Thus someone whose height is 5 ft. (152 cm) clearly is not tall, someone 5 ft. 8 in. (173 cm) is more tall than not tall, and someone 6 ft. 2 in. (188 cm) is clearly tall. It is important to realize that although the membership function might be defined precisely, it does not follow that the concept itself is precise over the domain.

Membership in a fuzzy set (as in a nonfuzzy set) is specified by a mapping from the universe to the set in question. This mapping may be performed by enumeration, by a function, by an algorithm, and the like. Whatever the method, the result will be that every element in X will have associated with it a number corresponding to its grade of membership in that fuzzy set. Note that the relations among the elements (or among relevant dimensions of the elements) need not specify a continuum. Where the relevant relations among the elements cannot be ordered, the mapping onto the fuzzy subsets would be by enumeration or an algorithm. However, where at least an ordinal relationship of elements (along one or more relevant dimensions) is implied, the grade of membership may be determined by a function, as well as by enumeration or an algorithm. Once this mapping is specified, the set can be used as a linguistic variable in fuzzy inferences and algorithms and can be modified by operations such as negation and union.

In both fuzzy and nonfuzzy set theory the *support* of the set A is the set of elements in X for which $f_A(x) > 0$. A (fuzzy or nonfuzzy) *singleton* is a set whose support is a single element in X. Therefore, a fuzzy set A may be specified as the union (see Equation 10) of its constituent singletons. Thus A may be represented by:

$$A = \int_X [f_A(x), x], \qquad (2)$$

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[1] For directness of exposition, concepts are assumed to be context constant (but not context free) here and throughout the study. In terms of membership functions, this implies that although the functions most likely contain more than one independent variable, the context-relevant variables have been held constant. For example, the membership function for *tall* is obviously influenced by the perspective (i.e., height) of the respondent. Assuming a constant perspective here in no way limits the applicability of the fuzzy set approach. (But see Labov, 1973, for a preliminary investigation of the interaction of form and context in referential meaning.)

where the integral sign represents the union of the fuzzy singletons $[f_A(x), x]$. If the fuzzy set A has finite support (i.e., if there are a finite number of elements whose grades of membership are greater than 0), then Equation 2 may be replaced by the summation:

$$A = [f_A(x_1), x_1] + [f_A(x_2), x_2] + \ldots + [f_A(x_n), x_n], \quad (3)$$

or

$$A = \sum_{i=1}^{n} [f_A(x_i), x_i], \quad (4)$$

in which $f_A(x_i)$ is the grade of membership of x_i in A. Note that the + sign in Equation 3 specifies the union of elements (see Equation 10) and not the algebraic sum. For example, given that the domain is the natural numbers, a fuzzy set labeled *few* might be defined as:

$$few = (.2, 1) + (.2, 2) + (.8, 3) + (1.0, 4) + (1.0, 5) + (.8, 6) + (.5, 7), \quad (5)$$

where the support of *few* is the set of numbers 1, 2, . . ., 7; that is, according to the definition, numbers greater than 7 have a grade of membership of .0 in the set labeled *few*. Notice that each ordered pair relates a natural number to the grade of membership of that number in the concept. This relationship may be extended by having the grade of membership itself be a fuzzy set. For example (from Zadeh, 1973), if the universe, X, is:

$$X = \text{Tom} + \text{Jim} + \text{Dick} + \text{Bob}, \quad (6)$$

and A is the fuzzy set *agile*, then:

$$agile = (\text{medium, Tom}) + (\text{low, Jim}) + (\text{low, Dick}) + (\text{high, Bob}). \quad (7)$$

Low, in turn, is a fuzzy subset of the universe of possible grade of membership values (referring mainly to values near .0), or:

$$low = (.5, .2) + (.7, .3) + (1.0, .4) + (.7, .5) + (.5, .6). \quad (8)$$

A *crossover point* in A is defined as an element that possesses a grade of membership in A of .5. Thus, *penguin* might be a crossover point in the set of birds, meaning that a penguin is as much a bird as it is not a bird.

Two fuzzy sets, A and B, are *equal* ($A = B$), if and only if $f_A(x) = f_B(x)$ for all x in X. Where the grades of membership take on values of either 0 or 1, this relation reduces to the traditional set theory definition of the equality of two sets. A is *contained* in B (or B *entails* A, or A is a fuzzy subset of B, i.e., $A \subset B$) if and only if $f_A(x) \leq f_B(x)$ for all x in X.

Operations on Fuzzy Sets

Negation, logical and algebraic operations, hedges, and other terms that influence the representation of linguistic variables can be considered as labels of various operations defined on fuzzy subsets of X. The more basic operations will be reviewed here. For a more complete overview, see Zadeh (1965, 1973) and Goguen (1967, 1969).

The *complement* of a set A is denoted *not* A, or $\bar{A}$, and is defined as:

$$not\ A = \int_X [1 - f_A(x), x], \quad (9)$$

that is, as in nonfuzzy set theory, the operation of complementation corresponds to negation. It is interesting to note that Black's (1937) reciprocal model of negation can be shown to be isomorphic to this complement operation.

The *union* of two fuzzy sets, A and B, is denoted $A + B$, and is defined as:

$$A + B = \int_X [f_A(x) \wedge f_B(x), x], \quad (10)$$

where

$$f_A(x) \wedge f_B(x) = \max[f_A(x), f_B(x)]. \quad (11)$$

The union of two sets corresponds to the logical *or* operation. In fact, in the situation where the grades of membership take on values of only 0 and 1, Equation 10 reduces to the Boolean *or* operation of set theory.

TABLE 1

LABELS OF FUZZY SETS AND THEIR PREDICTED OPERATIONS

Label	Operation
SMALL	f_{small}
LARGE	f_{large}
NOT SMALL	$1 - f_{small}$
NOT LARGE	$1 - f_{large}$
VERY SMALL	f_{small}^2
VERY LARGE	f_{large}^2
NOT VERY SMALL	$1 - f_{small}^2$
NOT VERY LARGE	$1 - f_{large}^2$
VERY VERY SMALL	$f_{small}^4 = f_{very\ small}^2$
VERY VERY LARGE	$f_{large}^4 = f_{very\ large}^2$
NOT VERY VERY SMALL	$1 - f_{small}^4$
NOT VERY VERY LARGE	$1 - f_{large}^4$
EITHER LARGE OR SMALL	$\max[f_{large}, f_{small}]$

The *intersection* of two fuzzy sets is denoted $A \cap B$ and is defined as:

$$A \cap B = \int_X [f_A(x) \wedge f_B(x), x], \quad (12)$$

where

$$f_A(x) \wedge f_B(x) = \min[f_A(x), f_B(x)]. \quad (13)$$

The intersection corresponds to the logical connective *and*, which also reduces to the Boolean operator for grades of membership of 0 and 1.

The above descriptions serve to demonstrate how various linguistic operators can be defined in terms of fuzzy sets. Zadeh (1972) and Lakoff (1973) have attempted to demonstrate that various linguistic operators (e.g., very, rather) can likewise be incorporated into the system of fuzzy logic by being considered as additional operators upon linguistic variables. For example, the adverb *very* appears to act as an intensifier. Zadeh reasoned that given a fuzzy set labeled A, *very* A should be of the form:

$$very\ A = \int_X [f_A{}^2(x), x]. \quad (14)$$

This formulation was later generalized to:

$$very\ A = \int_X [f_A{}^a(x), x],$$

$$\text{where} \quad a > 1.0. \quad (15)$$

Both Lakoff and Zadeh further analyzed operations that were concatenations of the primitive operations. For example, given the definitions of *not* (Equation 9) and *very* (Equation 14), the operation of *not very* A implies:

$$not\ very\ A = \int_X [1 - f_A{}^2(x), x]. \quad (16)$$

As part of the axiomatic system of fuzzy set theory, Zadeh (1968) generalized to fuzzy set theory the relation between the probability (P) of an event A, and the expected value of its membership function, that is:

$$P(A) = E(f_A). \quad (17)$$

Equation 17 has certain direct psychological implications. For example, the application of a term to an element might imply a grade of membership between .0 and 1.0. However, the overt judgment of the relation of the term and element might require a binary (yes–no) reply. Using Equation 17, the distribution of binary responses can be related to the grade of membership of the element in the fuzzy set labeled by the term. A technique is thus available for empirically verifying the predictions of fuzzy set theory in terms of the implications for human information processing.

Psychological Implications

Zadeh (1973) proposed that in dealing with humanistic systems we apply the

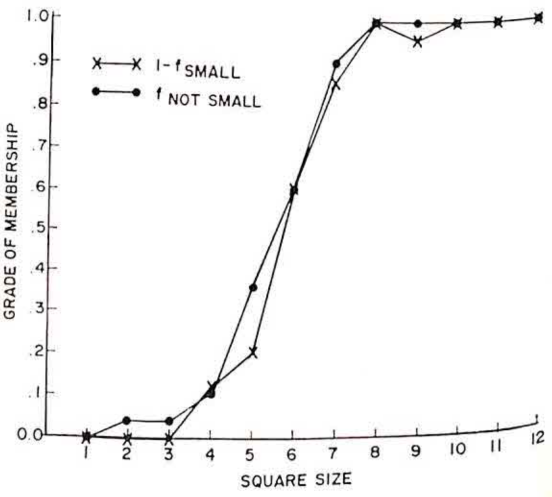

FIGURE 2. Membership function for *not small*.

principle of incompatibility: "As the complexity of a system increases, our ability to make precise and yet significant statements about its behavior diminishes until a threshold is reached beyond which precision and significance (or relevance) become almost mutually exclusive characteristics" (p. 28). Given the complexity of the system that psychologists study, one need not search far in order to appreciate the applicability of the above principle to the study of behavior.

This principle can actually be applied from two separate viewpoints. The first is in the description and explanation of behavior and in data analytic techniques. Some work has already been started in such areas as role theory (Thomason & Marinos, 1972), pattern classification (Bellman, Kalaba, & Zadeh, 1966), and clustering techniques (Ruspini, 1970; Bezdek, Note 3). The second view is in describing and explaining how people interact with the world around them. While both lines of research could profit greatly from utilizing these new quantitative techniques provided by fuzzy set theory, we have chosen the latter line of inquiry.

It was shown in the section on vagueness that people have the ability to comprehend and manipulate vague concepts. If this behavior could be described in terms of fuzzy logic, this new quantitative technique might motivate the development of new methods for the modeling of language comprehension, reasoning, natural problem solving, and other complex behavioral processes. To be more specific, the theoretical position that runs through this line of research is that people comprehend vague concepts (i.e., all natural language concepts) *as if* the concepts are represented as fuzzy sets. Moreover, people manipulate vague concepts *as if* they are processing according to the rules of fuzzy logic. Several preliminary studies appear to support this theoretical position.

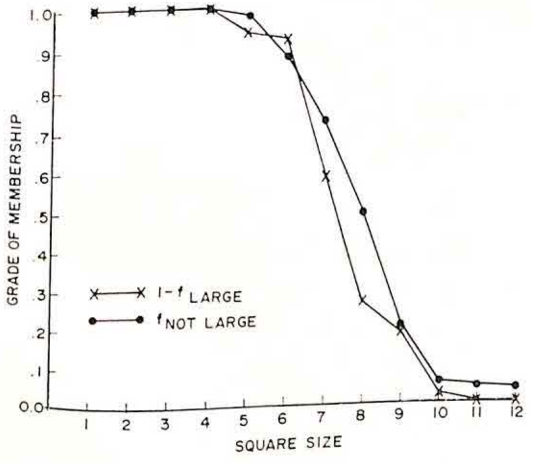

FIGURE 3. Membership function for *not large*.

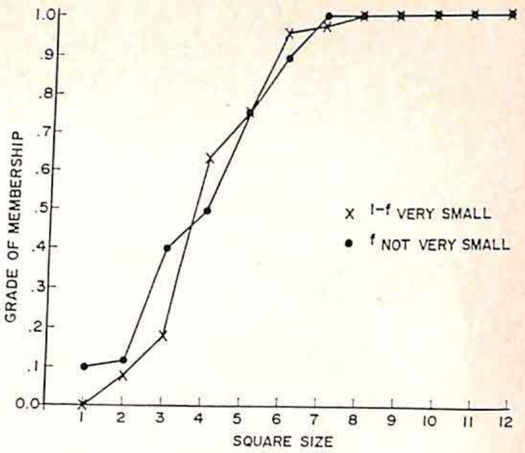

FIGURE 4. Membership function for *not very small*.

EXPERIMENT 1

Since Zadeh (1972) and Lakoff (1973) both make explicit claims as to the manner in which fuzzy sets should be transformed, it was felt that a logical first step would be to obtain some baseline fuzzy sets and empirically evaluate the transformations that occur when these sets are operated upon by negation and the addition of hedges.

Method

Subjects. Nineteen undergraduates at The Johns Hopkins University served as paid subjects.

Stimuli. Twelve slides, each containing a black square on a white background, were used as the physical stimuli. When projected on the screen, the squares measured 4, 6, 8, 10, 12, 16, 20, 24, 28, 32, 40, and 48 in. (10.2 cm to 121.9 cm) on one side.

Labels of the fuzzy sets consisted of the adjectives *large* and *small* paired with various combinations of *not* and the intensifier *very*. In addition, in order to test the concept of fuzzy union, the phrase

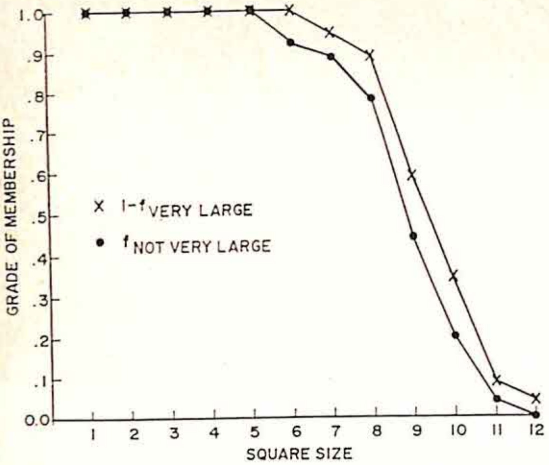

FIGURE 5. Membership function for *not very large.*

either large or small was also used. The 13 labels are shown in Table 1, along with the predicted transformations from Zadeh (1973).

Procedure. Subjects were run in one group. Each received an answer sheet containing the 13 phrases in one of 10 random orders. Below each phrase were 12 spaces. Subjects were instructed to look at the first phrase. They were told that they would be shown 12 squares in a random order. They were to simply look at each square and decide whether the phrase applied to it. If it was appropriate, they were to enter *yes* in the appropriate space; if it was not appropriate, they were to enter *no.* This procedure was repeated for each phrase, with a different random order of squares in each block. Slides were exposed for 1 sec. Subjects were given additionally 5 sec in which to respond. Before the start of the experiment the 12 squares were shown in ascending order to insure that everyone was operating within approximately the same context.

After every block of 12 slides, subjects were given a 30-sec break. After all 13 blocks, they were given an additional 5-min. rest, and the process then was repeated with different randomizations. Thus every subject responded twice to every combination of square and phrase. The entire session lasted 50 min.

Results

Since Zadeh (1968) had equated the probability of a fuzzy event with the expected grade of membership of the event, the proportion of *yes* responses for a particular square and phrase was interpreted as the grade of membership for that square in the fuzzy set labeled by the phrase. (The data were scaled by the method of successive intervals [Diederich, Messick, & Tucker, 1957] to obtain a psychological scale for the square size. Since the use of this scale in no way changed any of the results, the data are reported simply in terms of the ordinal square size.)

Figures 2 and 3 show the membership functions for *not small* and *not large,* respectively. In each graph the negative phrase is plotted with the complement of the corresponding affirmative phrase. In these figures, as in all remaining figures, the ordinate corresponds to the grade of membership (truth value), while the abscissa corresponds to the *ordinal* square size. Figures 4 and 5 relate *not very small* to the complement of *very small* and *not very large* to the complement of *very large,* respectively. Figures 6 and 7 show the corresponding relations for *very very small* and *very very large,* respectively. The fact that the graphs indicate a reasonably good fit, and the fact that the average root mean square error over all positive–negative pairs was less than .07 supports the fuzzy set notion of negation as being the complement of the positive set.[2]

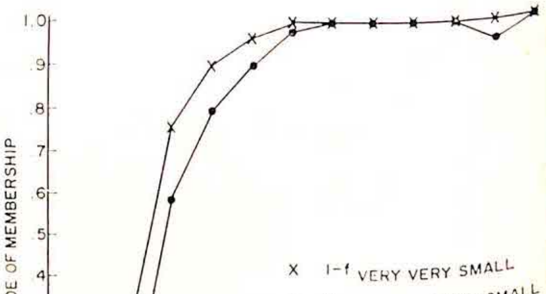

FIGURE 6. Membership function for *not very very small.*

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[2] Note that these results can likewise be predicted by a Thurstonian model where the concept is assumed to be constant and the evaluation of the square size is the variable component. Fuzzy set theory assumes that the response variability is mainly due to the vagueness of the verbal concept: The perception of square size is relatively constant. Differences at this point are more a matter of theoretical perspective than of quantitative prediction.

Figure 8 is a graph of *not large* and *small*. Although the two functions are monotonically decreasing, they do not represent the same concept. *Not large* appears to extend the concept *small* to include the midrange of the continuum.

The effect of the intensifier *very* on the concepts *small* and *large* are plotted in Figures 9 and 10, respectively. Note that the relation between *large* and *very large*, *very large* and *very very large*, and so on corresponds to the definition of entailment for fuzzy sets. Within each triplet (e.g., *small*, *very small*, *very very small*) the slopes of the functions appear approximately equal. (This was confirmed by the successive intervals analysis.) The equality of the slopes implies that the influence of *very* did not appear to reduce the vagueness of the concept. Now if Zadeh's (1972, 1973) hypothesis concerning the functioning of *very* as a power function is accurate, the slope of the function should increase as the intensifiers are concatenated. This does not seem to be the case.

Various classes of operators were tried in order to find a reasonable mapping function from a concept to the concept modified by *very*. Although a reasonable fit was obtained with a power function, it was felt that a more straightforward explanation was appropriate. In examining the plots of the membership functions it appeared that the addition of the intensifier *very* served to simply translate the function along the abscissa.[3] Support for this finding comes from the work of Cliff (1959), who found that adverbs tended to function as multiplicative constants. (Recall that Cliff was working under the assumption that the meaning of an adjective could be characterized as a point on a scale: The adverb functioned to shift the point along the scale.) A least squares method was used to obtain the best shift for *very* and *very very*. However, for ease of exposition, it is sufficient to describe the translation for *very* as two ordinal scale units and *very very* as an additional ordinal scale unit toward the extreme of the scale.[4] The results of this shifting operation are shown in Figures 11 and 12.

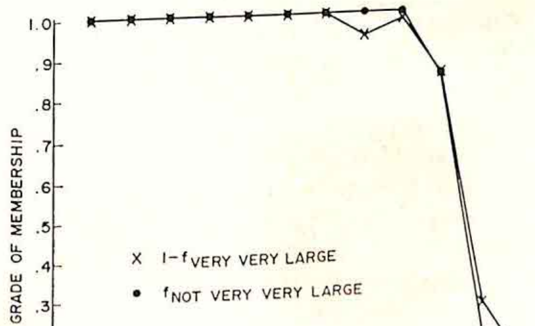

FIGURE 7. Membership function for *not very very large*.

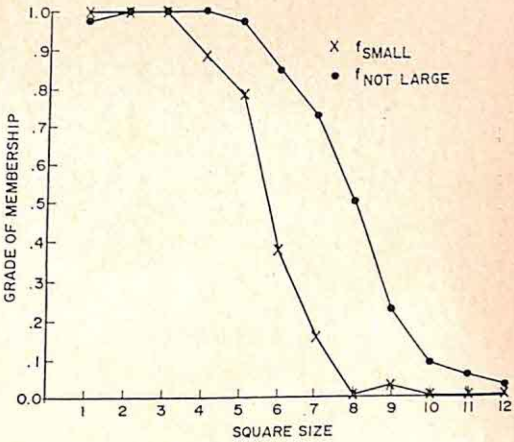

FIGURE 8. Membership functions for *not large* and *small*.

Figure 13 is a plot of the phrase *either large or small* and the operation of fuzzy union on the concepts *large* and *small*. While there is a definite similarity in the shape of the two functions, the discrepancies are more than minimal. The plot of the phrase is depressed in relation to the

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[3] Root mean square errors overall were .074, .214, and .061 for the best-fitting power, exponential, and translation functions, respectively.

[4] From the successive intervals scaling of the psychological square size, the ratio of the best translation to the mean intersquare distance was actually 1.81 and 1.15 for *very* and *very very*, respectively.

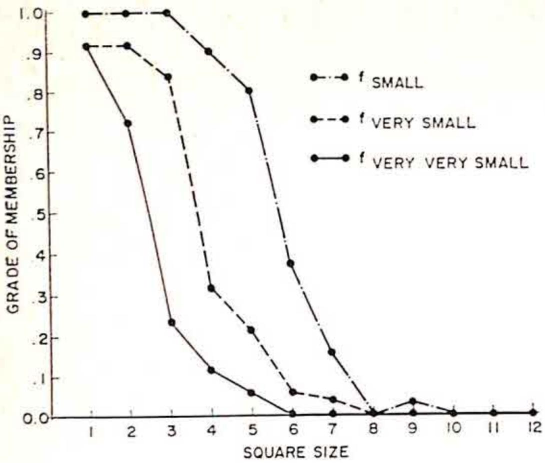

FIGURE 9. The effect of *very* on the concept *small*.

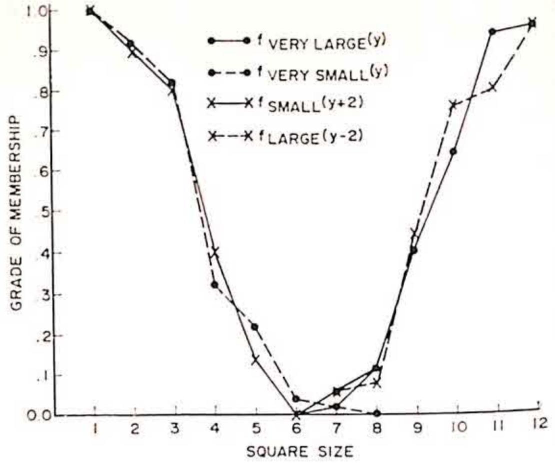

FIGURE 11. *Very* as a translation operation.

fuzzy set theoretic prediction. Some of the discrepancy is probably due to the difficulty that some subjects reported in evaluating this concept: the result being that the data contains possible inverted responses.

Overall, several conclusions are clear. Concepts defined by extension can be considered as fuzzy sets, and people tend to interpret these concepts and their variations in terms of operations on fuzzy sets. Negation can be characterized by the complement of the positive set. The effect of the intensifier *very* can also be interpreted as an operation on a fuzzy set, although its empirical form deviates from the predicted function. Operators like *not* and *very* can be concatenated with themselves and each other, with predictable results.

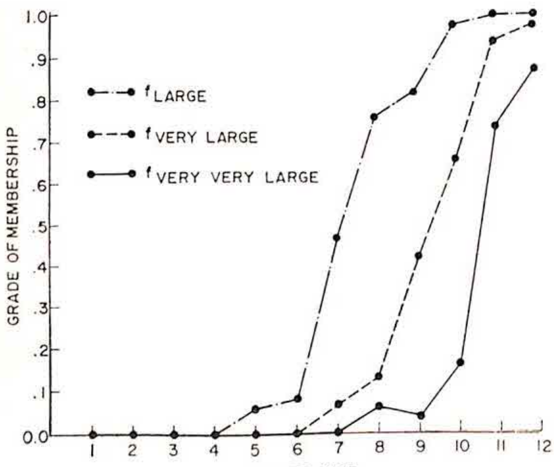

FIGURE 10. The effect of *very* on the concept *large*.

It would have been nicer if Zadeh's (1972, 1973) hypothesis of *very* operating as a power function of the grade of membership was applicable. Although such a finding would have supported the generality of the operation of *very*, the form of the operation is an empirical question. *Very* defined as a translation operation is obviously class dependent: It can certainly be generalized to other relative adjectives (e.g., *good*, *short*, *hot*). How such an intensifier would operate on the class of absolute adjectives (e.g., he is *very* British, this is *very* red) remains to be determined. Intuitively, it appears that the meaning of *very* in *very large* is qualitatively different from *very* in *very British*. The former implies an extreme of a continuum; the latter implies a greater emphasis on characteristic features (Lakoff, 1973).

The most important point of Experiment 1 is that no longer is the variability about the mean of a distribution considered to be strictly the result of noise in the system or the uncontrollable influence of extraneous variables. The variability has a specific interpretation in terms of the grade of membership of the element in the fuzzy set denoted by the concept label.

EXPERIMENT 2

Experiment 1 demonstrated that the composite responses of a group of subjects

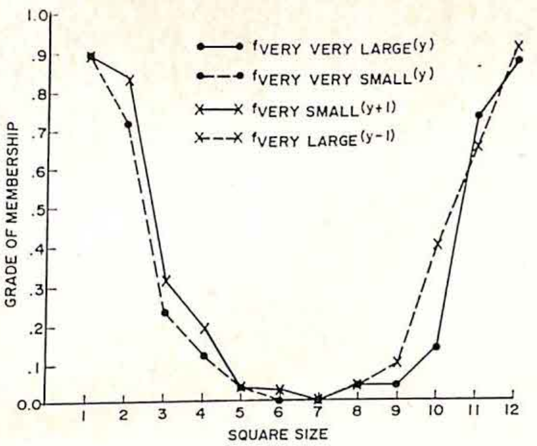

FIGURE 12. *Very very* as a translation operation.

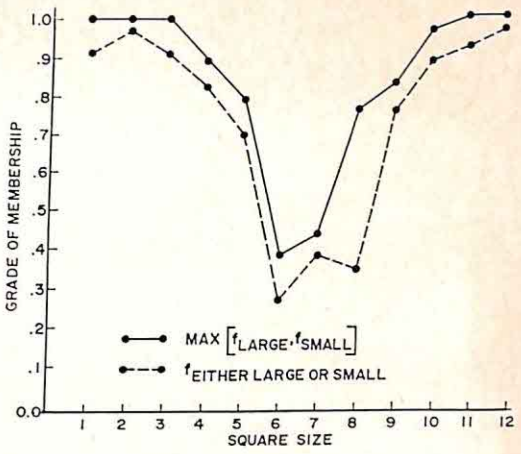

FIGURE 13. Membership function for *either large or small.*

could be considered as reflecting operations on fuzzy sets. The argument would be more convincing if it could be shown that individual subjects display similar behavior. Experiment 2 therefore can be considered a replication of Experiment 1 with repetitions across days for a single subject rather than across subjects for a single presentation.

Method

Subjects. Four students at The Johns Hopkins University, who had not been in Experiment 1, served as paid subjects.

Procedure. The experimental procedure was similar to Experiment 1. Exposure time for the slides remained at 1 sec; however, the intertrial intervals were under subject control. In order to obtain more information from each trial, confidence ratings were also used. Subjects were instructed to respond *yes* or *no* according to whether the square viewed was appropriate for the phrase. After this response was recorded, subjects rated how confident they were in their decision on a 5-point scale, where 1 was interpreted as purely guessing and 5 as absolutely certain.

In a typical session, which lasted less than 1 hr., a subject judged all combinations of the 12 squares and 13 phrases twice. Each subject participated in five such sessions over 5 days.

Results

The binary decisions and the confidence ratings were integrated into a single scale over the closed interval .0 to 1.0. For example, a *no* judgment with a confidence rating of five was assigned .0, while a *yes* response with a similar confidence rating was assigned 1.0.[5] Although there is no clear theoretical justification for this mapping, the resulting scale seemed to reflect the important conceptual ideas implicit in fuzzy set theory. The extremes of definite membership and nonmembership correspond to judgments that were rated as highly confident. Likewise, judgments rated

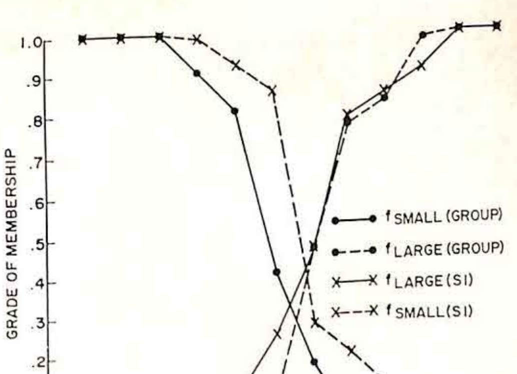

FIGURE 14. Membership functions from Subject 1 and the group data.

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[5] More formally, if d is the binary applicability decision ($1 = \text{yes}$, $-1 = \text{no}$) and r is the value on the confidence scale ($1 \leq r \leq 5$), then the grade of membership is defined as:

$$Grade = 5. + d\left(\frac{r}{10}\right).$$

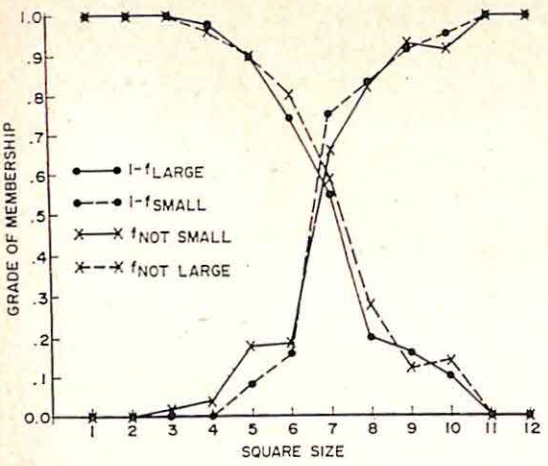

FIGURE 15. Membership functions from Subject 1 for *not large* and *not small.*

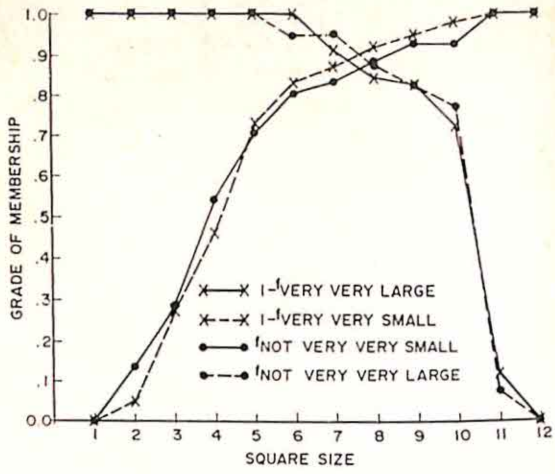

FIGURE 17. Membership functions from Subject 1 for *not very very large* and *not very very small.*

as purely guessing corresponded to the crossover points in a fuzzy set.[6]

The results of three of the four subjects resembled, with minor differences, the results of the group experiment (Experiment 1). Moreover, since there were no important differences among Subjects 1–3, only Subject 1 will be discussed. Subject 4 was quite different and will be discussed separately.

The results of Subject 1 are shown in Figures 14–20. Figure 14 compares the membership functions for *large* and *small* with similar functions obtained from the group experiment (Experiment 1). Although the functions for *large* appear very similar, the concept *small* is more extensive for Subject 1 than for the group. Information of this type might be useful in studies on individual differences in comprehension, but is irrelevant for the theory being considered. The hypothesis being tested is that the operations on fuzzy sets should remain constant, independent of the basic membership functions.

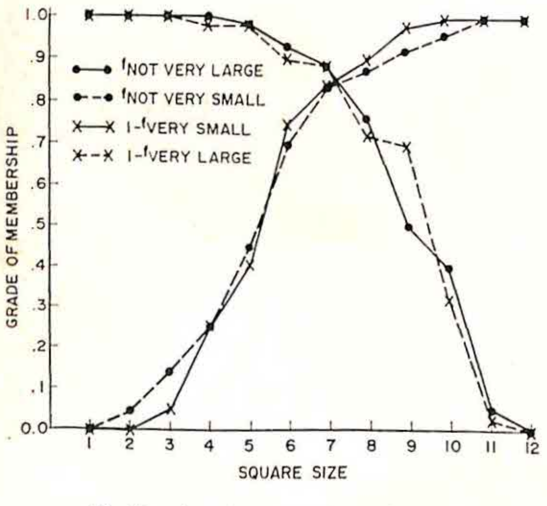

FIGURE 16. Membership functions from Subject 1 for *not very large* and *not very small.*

Figures 15, 16, and 17 compare the negative concept to the complement of the corresponding positive concept. As with Experiment 1, the functions are quite similar, implying that the effect of negating a concept is to replace the grades of membership with their complements.

Plots of *very* as a shifting operation are shown in Figures 18 and 19. As with the group data, the goodness of fit indicates that the translation operation is a tenable description of the effect of modifying a concept by *very*. Finally, Figure 20 shows that the membership function for *either large or small* shows a slight depression in relation to the fuzzy union of *large* and *small*, as was found in Experiment 1.

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[6] Inherent in this transformation is the assumption that equal confidence intervals reflect equal differential grades of membership. The comparison of the concept *large* for the group and for the individual subject, here Subject 1 (see Figure 14), tends to support this assumption.

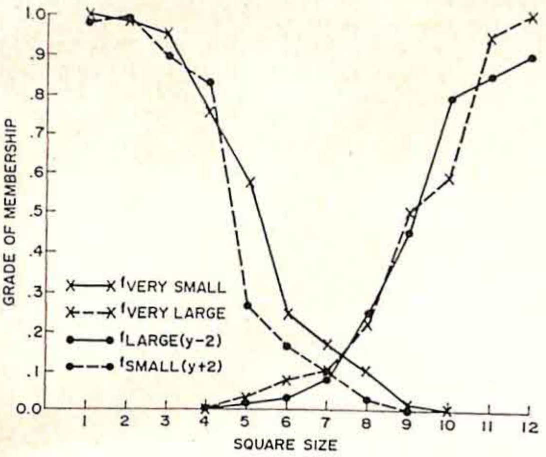

FIGURE 18. *Very* as a translation operation. (Data are from Subject 1.)

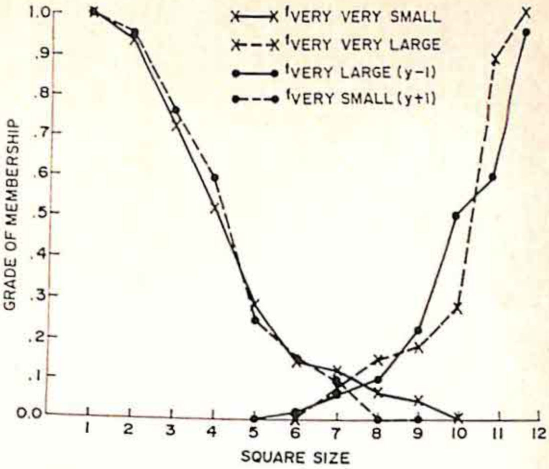

FIGURE 19. *Very very* as a translation operation. (Data are from Subject 1.)

Of the 3 individual subjects and the 19 group subjects tested, all responded in a similar manner. Their judgments all reflected fuzzy logical operations where the concept appeared to dichotomize the continuum (in a fuzzy manner) into a region of inclusion and a region of exclusion. Although these findings do reflect the assumptions of fuzzy set theory—from the perspective of linguistic processing—some of the results are rather anomalous. According to Bolinger (1972), the use of litotes such as *not very* does not imply the complement of the continuum as Figures 4 and 5 show, but only some range near the middle of the continuum: For example, to say that someone is *not very tall* is typically interpreted as meaning that he is *rather short* or *sort of short*. Subject 4 made judgments drastically different from the other subjects and seemed to be responding according to this more "idiomatic" interpretation of the phrases.

Figures 21 and 22 show the various positive membership functions as perceived by Subject 4. Whereas the other subjects perceived the operation of *very* on the concept *small* as defining successively smaller fuzzy subsets, Subject 4 interpreted the phrases as (fuzzy) overlapping categories. A square that is judged to have a maximum grade of membership in the set *very very small* is considered to be a marginal ($f = .56$) member of the set *small*. From the shape of the membership functions in Figures 21 and 22 it can be argued that the concept of strict entailment was not functioning for this subject. The fact that a square is judged to be *very very large* does not entail that it is also *large*.

The membership functions for *not small* and *not large* are presented in Figures 23 and 24, respectively, along with *large* and *small*. It appears that the two negative terms are rather general concepts representing the middle region of the contextual range. Functions for *not very small* and *not very large* are plotted in Figures 25 and 26, respectively. The functions seem to indicate that for this one subject the term *not* operates upon the intensifier *very* rather

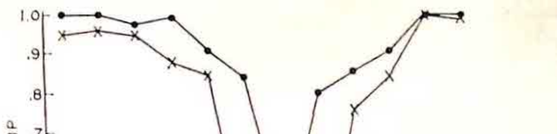

FIGURE 20. Membership function from Subject 1 for *either large or small*.

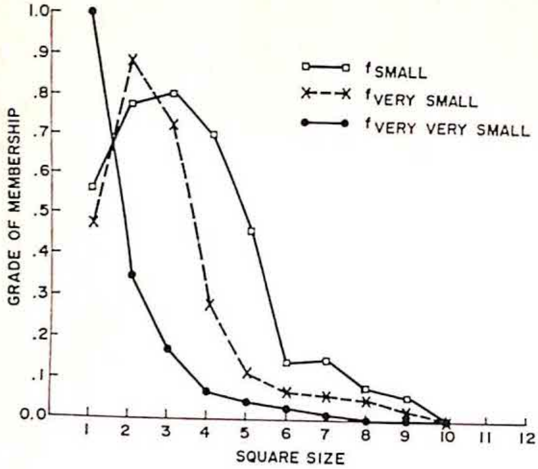

FIGURE 21. Membership functions containing *small* for Subject 4.

than on the entire concept. More explicitly, the original empirical formulation for *not very small* is:

$$f_{not\ very\ small}(x) = 1 - f_{very\ small}(x) \tag{18}$$

$$= 1 - f_{small}(x + d), \tag{19}$$

where d is the amount of translation on the abscissa. Over at least a portion of the domain, a tentative hypothesis for the behavior of Subject 4 might be:

$$\begin{aligned} f_{not\ very\ small}(x) &= f_{very\ small}(x - 2d) \\ &= f_{small}(x - d), \end{aligned} \tag{20}$$

that is, the function of *not* operates on the function *very* by translating the function about the base membership function. This formulation is extremely speculative, and the data to support the operation comes from only one subject out of 23. Nevertheless, it must be recognized that the empirical functions produced by this one subject, far from being random, appear to obey lawful relationships.

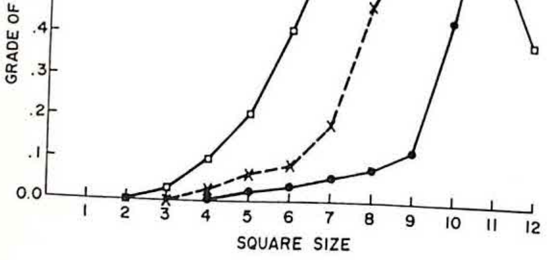

FIGURE 22. Membership functions containing *large* for Subject 4.

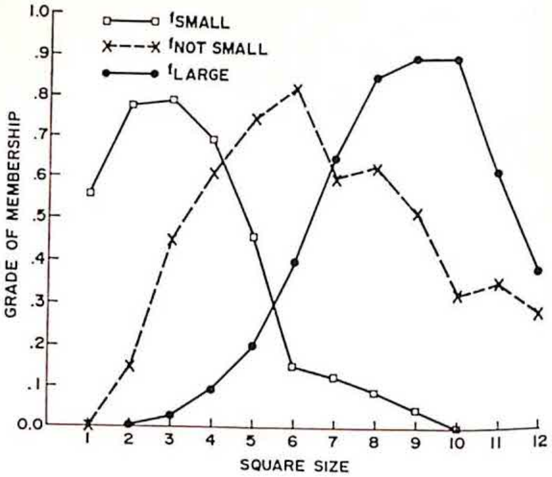

FIGURE 23. Membership function from Subject 4 for *not small*.

EXPERIMENT 3

The question remained as to why 22 subjects would respond in a (fuzzy) logically predictable manner, and only one subject respond in a manner that somewhat reflected a linguistic interpretation. Perhaps the demands of Experiment 2 were such as to force Subject 4 into responding in the more logical manner. To test this hypothesis Experiment 3 was performed; the context was varied in the hope that a more linguistic interpretation would result.

Method

Subjects. Fourteen undergraduates at The Johns Hopkins University, not involved in the previous experiments, participated as paid subjects.

Stimuli. The 12 squares used in Experiments 1 and 2 served as the physical stimuli. The terms were composed of the adjectives *large* and *small*, which appeared unmodified and in combination with *not*, *very*, and *not very*. In addition, the hedge *sort of* was included in order to evaluate the relation between *not very* and this relaxive adverb.

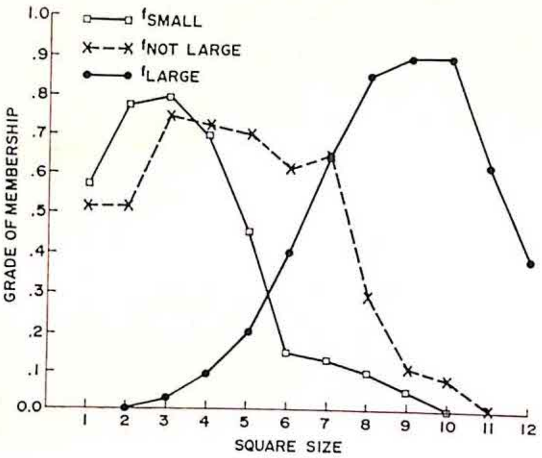

FIGURE 24. Membership function from Subject 4 for *not large*.

The terms were integrated into sentences of the form, "This is sort of small," which were then recorded in a random order onto one track of a stereo magnetic tape. The hope was that the auditory presentation of the sentences might be reflected in a more natural language interpretation. Two identical tapes were made. On Tape 1 the phrases *not very large* and *not very small* received strong emphasis on the word *not*. On Tape 2 the word *very* was emphasized. The remaining sentences were dubbed from the same master tape and thus were identical. On the second track of each stereotape synchronization information to automatically advance the slide projector was recorded.

Procedure. Subjects were randomly assigned to one of two treatment groups corresponding to which audiotape they would hear. The procedure for each group was identical. On each trial a slide would be projected. At the same instant that the slide was exposed, a sentence would be heard. The subject was to simply look at the slide and decide whether the sentence he was hearing was appropriate with reference to the square on the slide. Subjects circled the appropriate response (*yes* or *no*) on an answer sheet and indicated on a rating scale from 1 to 3 their degree of confidence in their decision. After 10 stimulus pairs there was a 15-sec pause, and after every 60 trials a 2-min. rest period. Each subject saw all combinations of the 12 squares and 10 sentences two times in a completely randomized order. Before they began, subjects were shown the entire range of slides in ascending order and the range of sentences, so they would all be operating in approximately the same context. The entire session lasted less than 45 min.

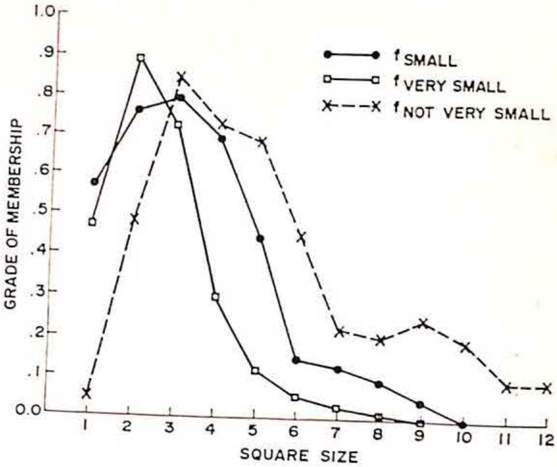

FIGURE 25. Membership function from Subject 4 for *not very small*.

Results

The findings of Experiment 3 demonstrate how robust the various concepts are under differing experimental conditions. Figures 27 and 28 show, respectively, the correspondence between *small* and *large* from Experiments 1 and 3. It is apparent that neither the change in modality (visual/auditory), emphasis, context (phrases/sentences), nor design (blocked/completely randomized) had any influence on the results. The functions for the other concepts correspond in a similar manner. The only other result of interest is the membership functions for *sort of small* and *sort of large*. These functions are plotted in Figures 29 and 30, respectively, and compare favorably with the plots for *not very small* and *not very large* for Subject 4.

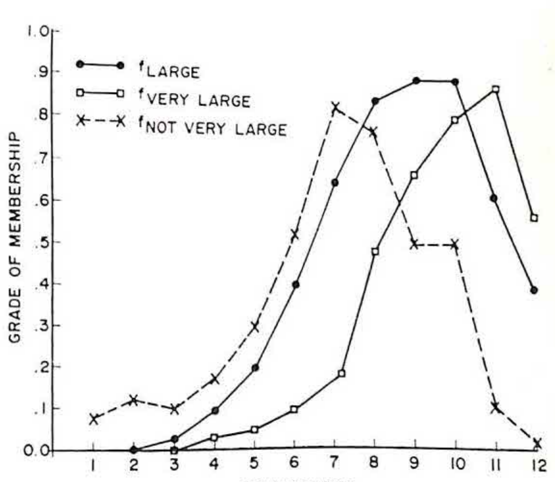

FIGURE 26. Membership function from Subject 4 for *not very large*.

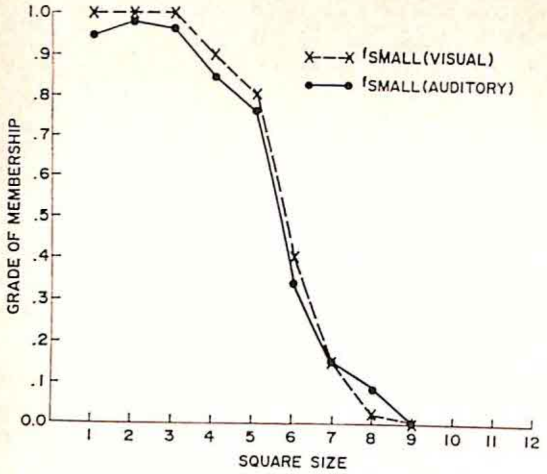

FIGURE 27. Membership functions for *small* from Experiment 1 (visual stimuli) and from Experiment 3 (auditory stimuli).

EXPERIMENT 4

In Experiments 1–3, having to make judgments on more than one phrase may have led subjects to treat concepts and modifiers in the observed "logically" predictable manner. This may result from subjects redefining the phrases in relation to each other. That is, the subjects may adopt a response strategy for specific phrases that is influenced by the other phrases used in the experiment. So, for example, the type of response to *not very small* may be a function of how the subject responded to *very small*. To overcome this possible contextual effect, an experiment was performed where each subject had to judge only a single phrase. It was felt that in this case, subjects might give a *linguistic* as opposed to *logical* interpretation to the phrases.

Method

Subjects. Forty-five undergraduates at The Johns Hopkins University, who had not participated in the earlier experiments, served as unpaid subjects. There were 10, 11, 12 and 13 subjects in the *large*, *very large*, *not very large*, and *sort of large* conditions, respectively.

Stimuli. Twelve black squares, proportional in size to the squares used in Experiments 1–3, were mounted on a white background. The mounted squares were fastened in ascending order (left to right) to a blackboard in the front of a classroom. Below each square was a number corresponding to the ordinal ranking. Unlike the preceding experiments, only four phrases were used: *large*, *very large*, *not very large*, and *sort of large*.

Procedure. Subjects were given a sheet of paper containing a single phrase and were instructed to look at the 12 squares and write down the identifying number or numbers of squares that might correctly be characterized by the phrase written on their sheet. Experiment 4 lasted approximately 2 min.

Results and Discussion

Figure 31 shows the membership functions for the four phrases. Two results are important. First, there is a small but predictable decrease in the grade of membership of the largest square for the concept *large*. This result is consistent with a *linguistic* interpretation of *large*, where *large* is not strictly entailed by *very large*. Second, the phrase *not very large* is clearly not the complement of *very large*. Rather as predicted by the *linguistic* interpretation, it seems to be functioning as *sort of small* (compare Figures 30 and 31). What is important here is simply that *not very large* is not the complement of *very large*. These two results together suggest that subjects will tend to give *linguistic* interpretations to the phrases if experimentally induced context effects are eliminated. The question arises whether these results are inconsistent with the formalism of fuzzy set theory. We think not. At worst they will complicate the particular characterization of various

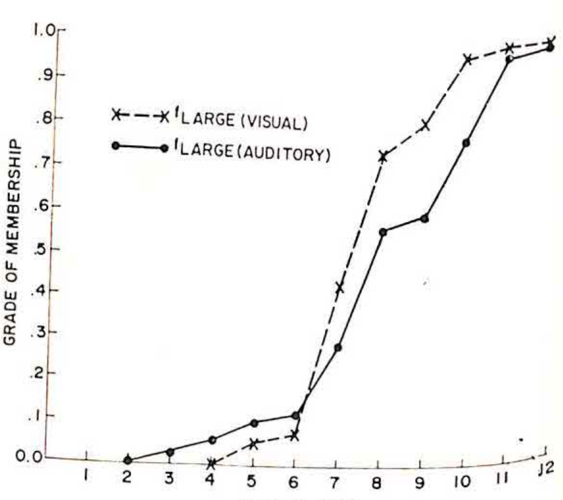

FIGURE 28. Membership functions for *large* from Experiment 1 (visual stimuli) and from Experiment 3 (auditory stimuli).

operators but not invalidate the principle claim that intensifiers and hedges can be described as operators on fuzzy sets. In fact, recall that we did give a formal, though tentative, formulation of the operations underlying the use of adverbs for the subject (Subject 4) who used a *linguistic* interpretation in judging the application of phrases to square size. The actual form that the operators and concepts take is an empirical issue to be determined by experimentation. Once we have determined the forms they take we can then proceed to see whether we can give them an interpretation within the framework of fuzzy set theory. This is essentially the procedure followed in the "odd" case of Subject 4 in Experiment 2.

General Discussion

The most important conclusion to be drawn from our results on the effects of operators *not* and *very* on the concepts *small* and *large* is that natural language concepts can be described more completely and manipulated more precisely using the framework of fuzzy set theory. A further, not uninteresting, finding was that concepts and operators can be interpreted in two different ways: what we have called the *linguistic* and *logical* interpretations. This latter finding is independent of any considerations relating to the treatment of concepts as fuzzy sets. It, however, does have important consequences for general psycholinguistic theory. Thus, it raises a cautionary note to be heeded by those who would treat certain lexical items as if they were strict logical operators. The results we have reported show that in certain natural language settings entailment does not strictly hold (e.g., *large* is not strictly entailed by *very large*) and that certain combinations of operators will assume an idiomatic sense very different from that predicted by a simple linear combination of the lexical items constituting the phrase (e.g., *not very large*).

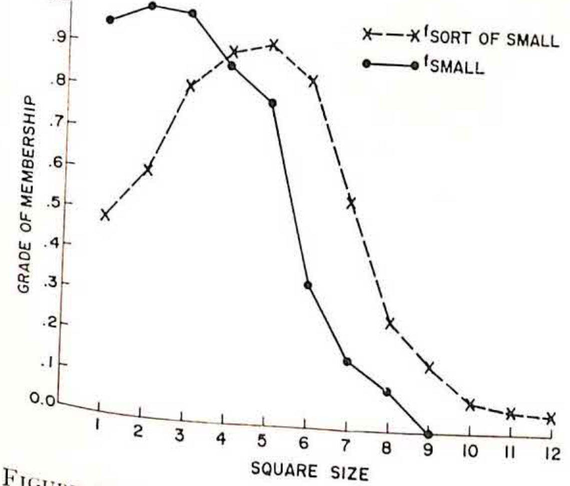

Figure 29. Membership function for *sort of small*.

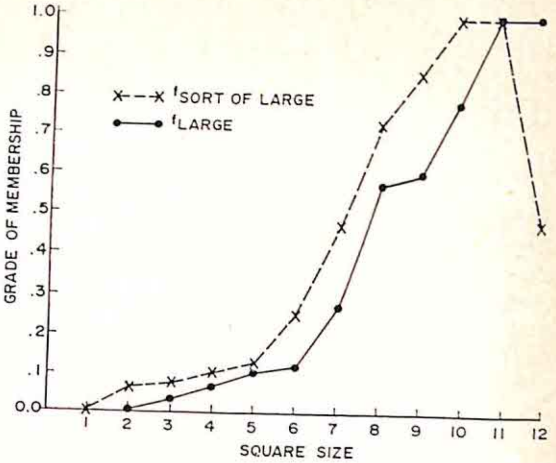

Figure 30. Membership function for *sort of large*.

As pointed out in the introduction, the implicit claim underlying our work is that natural language concepts are intrinsically vague, a view shared by an increasingly large number of psychologists, linguists, and philosophers. The treatment of natural language concepts as vague has important implications for semantic theory. The major implication is that the meaning of a term could be specified as a *fuzzy set of meaning components*. That is, a specific meaning component need not be necessarily either a member or a nonmember of the set of features that define the meaning of a term. All that is required in this view is that a meaning component have a nonzero degree of membership in the set. This approach, then, avoids a problem that has been the attention of a number of recently published reports where increasing em-

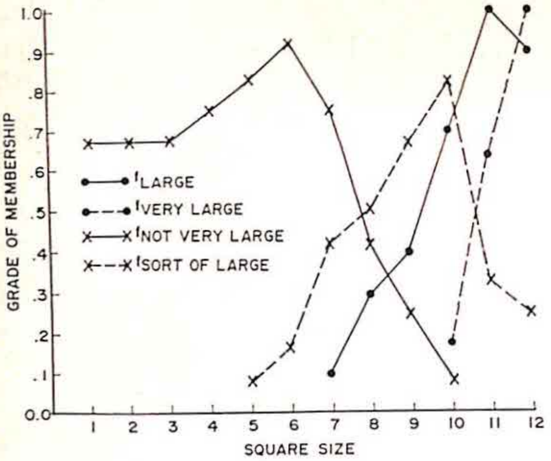

FIGURE 31. Membership functions resulting from the presentation of a single phrase to each subject (Experiment 4).

phasis has been placed on the problem of how to exactly determine what aspects of the meaning of a term are necessary and sufficient to characterize that term (Lehrer, 1970; Slote, 1966). The thrust of a number of empirical reports is that the traditional component approach to meaning (e.g., Katz & Fodor, 1963) incorrectly assumes that a specific component can be said, without any uncertainty, to be necessary for the definition of a term (Caramazza, Grober, & Zurif, in press; Garvey, Caramazza, & Yates, 1975; Lehrer, 1970; Rosch, 1973; Smith, Shoben, & Rips, 1974). More generally, the traditional componential approach is criticized for treating all components as contributing equally to the definition of a term. The alternative proposal emerging from these critical reports is that various components of meaning are differentially important to the definition of a term and, in addition, that no subset of these components can conclusively be said to be necessary and sufficient to define a term: That is, the semantic structure of a term itself is ill-defined, or vague. Though this latter approach may seem to be unnecessarily complicated, it allows for more natural explanations of important classes of semantic phenomena. Thus, for example, Labov (1973) has shown that attempts to give well-defined characterizations in terms of traditional componential analysis of the semantic structure of a common concept such as *cup* are inadequate. Labov obtained data on the consistency of naming various cuplike objects that support a theory in which various features are differentially weighted as necessary to define the term. The results Labov reported have been replicated in two independent studies with children as subjects (Anderson, 1975; DeVos & Caramazza, Note 4). In the study by DeVos and Caramazza (Note 4), it was shown that perceptual features of objects and functionally determined contexts interact in fairly specific ways to determine the label a child will assign to an object. The implication of this latter study is that from very early in life a child forms concepts that are vague (fuzzy), and contextual cues operate on perceptual features to determine the relative weighting the features are assigned in choosing a label (name) for the object. It is unclear how a traditional feature theory of meaning would handle such findings without recourse to complex ad hoc principles.

The importance of vagueness (fuzziness) as a basic concept in semantic theory can be seen by considering yet another semantic phenomenon: polysemy. Lexicographers have long recognized the fluidity of polysemy: No clear boundary distinguishes polysemy from homonymy. However, traditional feature theory requires that a definite demarcation hold between polysemous and homonymous senses—essentially marked by the presence of a specific feature. In fact, the defining property of polysemous words is that the senses that comprise a word share a common meaning core while at the same time differ sufficiently to be recognized as distinct from each other. In a recent investigation of polysemous words, it was shown, however, that the grade of membership of various senses in a general word concept vary considerably (Caramazza et al., in press). This finding is difficult to interpret within traditional feature theories, since they do not normally allow for features to be only partially applicable. This same finding can be easily interpreted within a theory that allows "vague" core meanings, where specific senses differ in degrees of membership to that core. Incidentally, it should be pointed out that Katz and Bever (Note 5) have criticized theories that postulate graded membership of instances in categories for confusing performance variability with inherent variability in concepts. They also make the blanket assertion that theories of this sort are based on empiricist principles. While not necessarily disagreeing with their characterization of Rosch's (1973) work, in particular, as having an empiricist flair, we find their wholesale labeling as "empiricist" theories that propose vague concepts to be rather naive. That is, there is no a priori reason to suggest that a rationalist theory necessarily excludes vague concepts.

To conclude, we are proposing that natural language concepts be considered as inherently vague and, specifically, as fuzzy sets of meaning components. We are also proposing that language operators—negative markers, adverbs, and adjectives—be considered as operators on fuzzy sets. Though the research we have reported in this article deals with a very narrow issue, modifying concepts, it does relate nonetheless in a direct way to semantic theory. Specifically, we have tried to give theoretical and empirical justification for the use of a formal system that can handle vagueness. The formal treatment of vagueness is an important and necessary (in our view) step toward a more comprehensive handling of natural language phenomena and the communication of vague information.

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A Fragmentation Hypothesis of Memory: Cued Recall of Pictures and of Sequential Position

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SUMMARY

It is a common assumption that memories consist of sets of interrelated features or attributes. Rigorous and direct investigation of the functional properties of these sets has been relatively neglected however. Two important questions are posed here. First, are the interrelationships of different components reflexive or are they asymmetric? Second, how do components interact when more than one is used as a cue for recall?

These questions were answered by examining memory for series of items whose objective components were statistically independent of one another. The items were color photographs which each depicted a particular object (e.g., a cup) of a particular color (e.g., yellow) in a particular location (e.g., the top left corner of the backdrop); the photograph also occurred in a particular sequential position of course (e.g., as the second photograph of a series). After a filled 25-sec delay, recall of each item was assessed using one, two, or three of these four components as cue for the remainder.

A fragmentation hypothesis was suggested to account for the results concerning the visual attributes. This postulates that a memory trace corresponds to a fragment of a perceived situation. A fragment can be excited by the provision of a single constituent as cue: Recall is therefore reflexive, and the cuing of a memory by several of its constituents is no more effective than that by a single one. Multiple cuing will usually produce rather higher overall recall simply because there is an increased chance that at least one of the cue components is stored in the corresponding fragment. Upon rigorous testing it was found that the hypothesis provided an accurate representation of recall. Two other models, an "independent cue" hypothesis and a model based upon that of Anderson and Bower (1973), were significantly discrepant with the data.

Sequential position did not conform to the fragmentation hypothesis, correct recall being more likely when sequential position was used as a cue for another component than when it was itself cued for. In the latter case the median recalled value of sequential position was related to the true value by a power function. An implication is that the lower level of recall usually occurring for items in the middle of a list is due to the increasing inaccuracy of the contextual component being used as the implicit cue.

It seems intuitively plausible, and theoretically viable (e.g., Bower, 1967b; Tulving & Thomson, 1973), to postulate that a "memory" corresponds to the mental storage of a set of interrelated components. Such a set may be "remembered" when one or more of its component attributes is subsequently provided as a "cue." Two fundamental aspects of this recall process are examined here. First, is cuing symmetric? Is the probability of recalling Attribute a when given Attribute b as cue the same as that of recalling Attribute b given a? Second, what is the effect of providing two independent cues for recall? How does the probability of recalling Attribute a when given both b and c as cues relate to the probability of recall given either b or c alone? A quantitative investigation of these questions was carried out which led to a general formulation of the nature of the memory trace.

The method used, as in the independent investigations of Anderson and Bower (1973, chap. 10) and Tulving and Watkins (1975) was a form of cued recall—in this case not of verbal material but of a series of naturalistic pictures. Each of the items to be remembered had four specific attributes. A *yellow cup* in the *top left* corner of the *second* picture in a series would be an example, containing the four components color, object type, location, and sequential position. Subjects were later cued for recall of each item by the provision of one or more of its attributes. They might, for instance, have been asked to recall the color and location of the *cup* and the sequential position of the picture which contained it. Alternatively they might have been cued by *yellow* and *second* and have had to attempt recall of the corresponding location and object type; or, cued by *cup*, *top left* and *second*, have had to recall color.

Sequential position was included to act as an explicit form of the contextual information usually implicit in the formation of recent memories. It has been suggested by Tulving (1968) and more recently by Baddeley and Hitch (1974) that it is possibly the declining accuracy of this type of information, rather than the mere passage of time, which gives rise to the recency effect (improved recall of the last few items in immediate free recall). If this were the case it would be expected that a "recency" effect could occur even after a considerable delay between presentation and recall if cuing were of an appropriate type.

Previous research has not resolved whether the recall process is fundamentally symmetric. Overt performance has been shown to be asymmetric in certain circumstances, with nouns, for example, generally acting as better cues for adjectives than vice versa (Horowitz & Manelis, 1972; Lockhart, 1969). Asch and Ebenholtz (1962), however, demonstrated approximate symmetry in the recall of two-component visual patterns and argued that asymmetry in other circumstances derived from differential "availability" of the components. The present experiment sought greater generality by investigating the simultaneous interrelationships of four different components.

The effect of providing more than one recall cue has been investigated by using as cues for a "target" word either common free associates of it (McLeod, Williams, & Broadbent, 1971; Tulving & Osler, 1968) or rhyming or semantically related words (Tulving & Watkins, 1975). Apart from its functional asymmetry, such a procedure has the disadvantage that there is a non-negligible and variable chance of correctly generating a target word by guessing alone, especially when more than one cue is provided. In the present experiment the different components are orthogonal, and therefore the known probability of generating a correct answer by guessing alone is the same for multiple cuing as for single cuing.

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A plausible "independent cue" hypothesis is that if there are several different cues for the same item they should each exert independent, additive effects. The observed efficacy of multiple cuing, however, is much less than this hypothesis predicts. Instead a new formulation of the structure of the recall process is proposed, and proves to provide an accurate quantitative representation of the results concerning the visual attributes. It is hypothesized that memories correspond to "fragments" of perceived situations, all-or-none clusters of components of the original stimuli: The hypothesis implies that the remembering process fundamentally is both symmetric and also facilitated by multiple cuing only insofar as this increases the likelihood of cue and fragment overlapping. The recall which directly involves sequential position constitutes an exception, however, with an additional explanation being necessary in this case.

Anderson and Bower (1973, chap. 10) have proposed a model, based upon their computer program HAM, for data of similar complexity concerning the cued recall of sentences. It has been suggested by Bower (Note 1) that the results of the present experiment may be represented by a model derived from that of Anderson and Bower. Bower's model proposes that each item is stored as a composite proposition, with each component having two characteristic probabilities: first of retrieving the proposition and second of being itself recalled when the proposition has been retrieved by other components. The fit of this model to the present data is examined here and shown to be less accurate than that of the fragmentation hypothesis.

METHOD

Subjects

The subjects of this experiment were 54 male students of Cambridge University who were each paid 50p for participating. Subjects had been preliminarily screened to ensure that they were not color-blind.

Stimuli

Subjects were shown sets of nine slides to remember. Each slide was a color photograph of a particular type of object (O), painted a particular color (C), in a particular spatial location (L). The nine objects consisted of a screwdriver, cup, comb, milkbottle, ball, scissors, fork, (toy) gun, and (toy) flute, while the nine colors were red, orange, yellow, green, blue, pink, gray, brown, and black. The setting for the objects was a simulated shop window, with background drapes and a surrounding window frame. The objects were displayed on three steps in the center of the window. The nine locations were specified by the elements of a 3×3 matrix: the left, center, and right of each of the top, middle, and bottom steps.

Each subject successively viewed and recalled nine separate sets (of nine slides each). Each pairing of attribute values occurred exactly once in these 81 slides, since the values of each of the above three dimensions were specified by one of three orthogonal 9×9 Latin squares (obtained by randomization of an example given by Fisher, 1949). Each set of nine slides corresponded to a particular row of this three-dimensional hyper-Graeco-Latin square, its slides being presented in an order corresponding to left-to-right progression across the row. Consequently, the sequential position (S) of presentation was a fourth independent and orthogonal attribute of each stimulus. It was used as a cue or as a feature to be recalled in exactly the same way as color, location, and object type were.

The order in which subjects were shown the nine sets of slides was varied, in case the level of recall of a set was dependent upon its position within the session or upon the immediately adjoining sets. These factors were controlled by presenting the sets in the 18 different orders of presentation specified by a double 9×9 balanced-predecessor Latin square (Williams, 1949). Each of these 18 orders was shown to three subjects tested individually or together.

Questions

After a delay, the recall of each set of items was tested. Subjects were given answer booklets, each page of which represented a particular item. Each page depicted the nine possible values of each of the four attributes: nine blocks of color, nine silhouettes of objects, nine locations marked on a drawing of the backdrop, and nine boxes in a row to represent the nine sequential positions. The pages were in a different order from that in which the slides were presented. On each page, the particular value of either one, two, or three of an item's four attributes had been marked by the experimenter. The subject had to attempt recall of the corresponding missing three, two, or one attributes.

Four of the nine items of each set were single cued (e.g., by C), three were double cued (e.g., by CS) and two were triple cued (e.g., by CLO). There are four single-cue types (C, L, O, S), six double-cue types (CL, CO, CS, LO, LS, OS), and four triple-cue types (CLO, CLS, COS, LOS). Consequently each subject was cued by each single-cue type on nine occasions, but by each double- or triple-cue type on only four or five occasions.

The position within the question series of each of the different cue types was balanced over sets. Two other aspects of the design of the questions were also controlled over sets, except for a small number of unavoidable exceptions for certain multiple-cue types. First, for all cue types each different value of each attribute was used as a cue once only. Second, the instances of each cue type were uniformly distributed over presentation sequential position, each different value of the sequential position being associated with each cue type once only. Thus the question design was balanced with respect to the possible effects of question order, individual attribute values, and presentation sequential position.

Procedure

Each subject was given a detailed set of instructions to read. The instructions fully informed the subjects of the nature of the task; in addition the experimenter paraphrased them informally. The subject was shown three preliminary slides in order to provide familiarity with the type of material to be used: first, a slide showing the shop window empty; second, one showing all nine object types, each in the same color (black); and third, one showing all nine colors, each painted on the same type of object (cups). In these last two slides the objects were placed in the nine different locations.

The slides were projected by a Kodak Carousel S-AV linked to an electronic timer. After a "get ready" slide, the timer controlled the automatic exposure of nine slides in succession. The cycle time was 2.2 sec (1.1-sec exposure, 1.1-sec dark interval). After the ninth slide, a tenth one appeared. This was a three-figure number (e.g., 461) which was exposed for 25 sec. Subjects counted aloud backward in threes from this number until it disappeared, when they commenced recall. The purpose of the delay interval was to avoid a relatively large difference in time between presentation and recall of early and late items of the list; counting backward in threes (Peterson & Peterson, 1959) obviated the possibly distorting effect of preferential rehearsal of certain of the list items. Recall was unpaced and took approximately 4 minutes for each list.

RESULTS

It is shown in this section that the data involving sequential position differed fundamentally from those involving the other, visual attributes. A fragmentation hypothesis concerning the patterns of recall of the visual attributes is proposed: Upon rigorous testing, its implications are found to be correct.

The terminology used is straightforward. Particular attributes are referred to as before by their initials in uppercase (e.g., L for location), while in general algebraic discussion the lowercase symbols a, b, c, and d are used in an undifferentiated manner. The probability of recalling Component a when cued by Component b is written $P(a|b)$: "a given b." Likelihood ratio analysis of contingency tables is employed; the resulting statistic, termed the *minimum discrimination information statistic* ($2\hat{I}$) by Kullback (1959, p. 85), is asymptotically distributed as chi-square (with an appropriate number of degrees of freedom).

Overall Conditional Probabilities

Table 1 shows the percentage of answers correct for each cue-type–answer-component combination. Apart from a few omissions, each percentage is based upon 9×54 (i.e., 486) observations for each single-cue type, and upon either 4×54 or 5×54 (i.e., 216 or 270) observations for the double- and triple-cue types. Table 1 illustrates two important features of the recall in this experiment: its partial symmetry and its holistic nature.

First, although pair-wise comparisons showed that the single-cued probabilities $P(a|b)$ and $P(b|a)$ were nonreflexive overall, $2\hat{I}(6) = 59.27$, $p < .001$, further analysis showed this asymmetry to be confined to the data involving sequential position, $2\hat{I}(3) = 56.51$, $p < .001$, for which $P(a|S)$ was greater than $P(S|a)$ in each case. In contrast, the conditional probabilities which involved only the visual attributes C, L, and O were reflexive, $2\hat{I}(3) = 2.76$, *ns*.

Second, using two (or more) cues for recall instead of just one produced surprisingly little improvement. This rules out an independent cue hypothesis: the possibility that the cues act in a statistically independent way. If this were the case, double-cued probabilities would be given approximately by $Q(a|bc) = Q(a|b)Q(a|c)$, where $Q(a|bc) = 1 - P(a|bc)$, etc. This is because the probability of two independently operating cues both failing to produce recall is the product of their probabilities of failure when used individually.

The above equation is not quite accurate since it neglects the fact that for both single and double cuing there is a base-level probability, g, of guessing the correct answer by chance. Since the guessing and remembering also are statistically independent, in each case we have observed $Q = (1 - g)Q_{\mathrm{T}}$, where Q_{T} represents a hypothesized level of "true" recall. Hence:

$$Q(a|bc) = (1 - g)Q_{\mathrm{T}}(a|b)Q_{\mathrm{T}}(a|c)$$
$$= \frac{Q(a|b)Q(a|c)}{1 - g}. \qquad (1)$$

It was assumed that guessing was randomly distributed over the nine different values of each component, and so g was set at 1/9. As an example, the expected $P(\mathrm{O}|\mathrm{CL})$ calculated from $P(\mathrm{O}|\mathrm{C})$ and $P(\mathrm{O}|\mathrm{L})$ (.417 and .370, respectively) is .587. This theoretical value is higher than the observed .472, which proved to be the case for all 12 double-cued probabilities. The independent cue hypothesis may be dismissed, therefore, since the binomial probability of so extreme a result is $2(1/2)^{12}$ (i.e., $p < .001$).

Fragmentation

The data of this experiment may be presented in a more detailed form than that of Table 1. Table 2 shows the frequency of occurrence of each possible pattern of correctly recalled components to individual cuings. Thus, for example, Pattern 1 represents the number of occasions when L, O and S were all recalled correctly when cued for by C, while Pattern 2 represents the correct recall of L and O but not S when cued for by C. Each class is also shown as a percentage of the total number of occurrences of the relevant cue type.

TABLE 1

OVERALL PERCENTAGE CORRECT FOR EACH CUE-TYPE–ANSWER-COMPONENT COMBINATION

Cue type	Answer C	L	O	S
C	—	32.8	41.7	19.8
L	31.2	—	37.0	27.4
O	44.3	41.3	—	24.2
S	31.1	41.9	36.7	—
CL	—	—	47.2	24.4
CO	—	43.3	—	30.2
CS	—	42.1	52.2	—
LO	42.4	—	—	34.6
LS	29.1	—	42.5	—
OS	41.7	46.0	—	—
CLO	—	—	—	39.1
CLS	—	—	59.1	—
COS	—	53.3	—	—
LOS	51.5	—	—	—

Note. C = color, L = location, O = object type, S = sequential position.

Inspection of Table 2 provides further evidence of symmetrical, holistic processing. In addition to the overall reflexivities examined in the previous section, the data concerning the visual attributes contain further approximate symmetries: For example, the frequencies of LO given C, CO given L, and CL given O are all approximately equal at 75, 79, and 85, respectively. Furthermore, the incidence of double- and triple-correct answers is very much greater than it would be if these occurred only by the chance conjunction of two or three independently correct answers.

The fragmentation hypothesis was formulated as a precise expression of a possible explanation of these aspects of the data. It is shown below that it does not apply to the whole corpus of data, but does hold rigorously for the three visual attributes; sequential position is considered separately later.

It is hypothesized that the memory trace corresponds to a *fragment* of the stimulus situation and may contain any or all of the attributes of explicit interest, as well as other attributes not tested in the experiment. Any attribute contained in the fragment, used as a cue, gives access to the remainder of the fragment.

To take the example of a fragment containing all three components C, L, and O, cuing with either C, L, or O will induce correct recall of the other two. It follows that, as observed experimentally, the probabilities of double-correct answers to single cuing, $P(ab|c)$, should be approximately equal for different cue types and may be large relative to the corresponding single-correct probabilities, $P(a|c)$ and $P(b|c)$. Furthermore, for this particular type of fragment the provision of, say, both

TABLE 2

Fragmentation Representation of the Composition of Recall for Each Cue Type

Pattern no.	Cue type	Correctly recalled	Data frequency	Data %	Fragment type(s)[a]	Theoretical %
1	C	LOS	42	8.92	1	9.89
2	C	LO	75	15.92	2	14.36
3	C	LS	14	2.97	3	2.35
4	C	OS	10	2.12	4	2.56
5	C	L	24	5.10	6, 12	4.60
6	C	O	67	14.23	7, 13	12.93
7	C	S	27	5.73	8, 14	4.84
8	C	zero	212	45.01	5, 9, 10, 11, 15	48.48
9	L	COS	32	6.84	1	9.89
10	L	CO	79	16.88	2	14.36
11	L	CS	9	1.92	3	2.35
12	L	OS	22	4.70	5	4.84
13	L	C	25	5.34	6, 12	4.60
14	L	O	40	8.55	9, 14	8.05
15	L	S	65	13.89	10, 13	11.15
16	L	zero	196	41.88	4, 7, 8, 11, 15	44.76
17	O	CLS	53	11.25	1	9.89
18	O	CL	85	18.05	2	14.36
19	O	CS	12	2.55	4	2.56
20	O	LS	18	3.82	5	4.84
21	O	C	58	12.31	7, 13	12.93
22	O	L	39	8.28	9, 14	8.05
23	O	S	31	6.58	11, 12	7.51
24	O	zero	175	37.15	3, 6, 8, 10, 15	39.86
25	S	CLO	86	18.57	1	9.89
26	S	CL	16	3.46	3	2.35
27	S	CO	19	4.10	4	2.56
28	S	LO	35	7.56	5	4.84
29	S	C	25	5.40	8, 14	4.84
30	S	L	57	12.31	10, 13	11.15
31	S	O	33	7.13	11, 12	7.51
32	S	zero	192	41.47	2, 6, 7, 9, 15	56.87
33	CL	OS	35	13.16	1, 4, 5, 13, 14	20.57
34	CL	O	90	33.83	2, 7, 9	32.06
35	CL	S	30	11.28	3, 8, 10	15.06
36	CL	zero	111	41.73	6, 11, 12, 15	32.31
37	CO	LS	47	21.86	1, 3, 5, 12, 14	21.91
38	CO	L	46	21.40	2, 6, 9	22.18
39	CO	S	18	8.37	4, 8, 11	10.07
40	CO	zero	104	48.37	7, 10, 13, 15	45.84
41	CS	LO	86	32.33	1, 2, 5, 12, 13	35.79
42	CS	L	26	9.77	3, 6, 10	11.40
43	CS	O	53	19.92	4, 7, 11	16.29
44	CS	zero	101	37.97	8, 9, 14, 15	36.52
45	LO	CS	50	19.01	1, 3, 4, 12, 13	21.50
46	LO	C	63	23.95	2, 6, 7	25.18
47	LO	S	41	15.59	5, 10, 11	16.80
48	LO	zero	109	41.44	8, 9, 14, 15	36.52
49	LS	CO	49	23.00	1, 2, 4, 12, 14	31.64
50	LS	C	13	6.10	3, 6, 8	6.95
51	LS	O	41	19.25	5, 9, 11	15.57
52	LS	zero	110	51.64	7, 10, 13, 15	45.84
53	OS	CL	52	24.19	1, 2, 3, 13, 14	29.87
54	OS	C	38	17.67	4, 7, 8	17.05
55	OS	L	47	21.86	5, 9, 10	20.77
56	OS	zero	78	36.28	6, 11, 12, 15	32.31
57	CLO	S	84	39.07	1, 3, 4, 5, 8, 10, 11, 12, 13, 14	43.13
58	CLO	zero	131	60.93	2, 6, 7, 9, 15	56.87
59	CLS	O	127	59.07	1, 2, 4, 5, 7, 9, 11, 12, 13, 14	60.14
60	CLS	zero	88	40.93	3, 6, 8, 10, 15	39.86
61	COS	L	144	53.33	1, 2, 3, 5, 6, 9, 10, 12, 13, 14	55.24
62	COS	zero	126	46.67	4, 7, 8, 11, 15	44.76
63	LOS	C	139	51.48	1, 2, 3, 4, 6, 7, 8, 12, 13, 14	51.52
64	LOS	zero	131	48.52	5, 9, 10, 11, 15	48.48

Note. C = color, L = location, O = object type, S = sequential position.
[a] See Table 3.

C and L as cues for O is redundant, since either alone would have been sufficient for recall to have occurred. Consequently the overall improvement with double cuing is generally likely to be slight.

Before the hypothesis is applied to the data, two simplifying points relevant to this experiment, but not necessarily others, should be noted. First, fragments may contain contextual components in addition to those components specified by the experimenter. In this experiment the cuing was designed to investigate the interrelationships only of the four explicitly specified components, but in the method of free recall, for example, contextual components which have not been explicitly specified by the experimenter may also be of importance. Second, since this experiment is concerned with the cue-recall relation between two or more components, fragments which contain only *one* of the explicit components may be disregarded, being subsumed in the null category of fragments which support no recall at all. Single-component fragments would, of course, be of interest if memory were tested by recognition.

With these simplifications, the 15 different types of fragment shown in Table 3 are relevant. The "twin fragments" 12, 13, and 14 express the possibility of two independent fragments being derived from the same item.

Table 2 shows the expected composition of the data in terms of these 15 different fragment types. Ignoring the effects of guessing for the present, it can be seen that some of the data can arise from only one type of fragment. For example (line 2) the correct recall of L and O but not S when cued with C implies a Type 2 CLO fragment. If all the data classes were uniquely specified in this way, the fragmentation hypothesis could be rigorously tested by a parameter-free method. If the hypothesis is correct, the inferred probabilities of occurrence of each of the 15 types of fragment would be constant over the 14 different cuing types. However, many of the fragments are not identifiable (cf. Greeno & Steiner, 1964), in the sense that the observed pattern of recall could have arisen from any one of several different types of fragment. For example (Pattern 34) the correct recall of O but not S when cued with both C and L could indicate the presence of a Type 2, 7, or 9 fragment (CLO, CO, and LO, respectively). Hence it would in practice be possible to test only a limited subset of the above symmetries.

Instead, to test the hypothesis's representation of the whole set of data, it is necessary to estimate the probability of occurrence of each of the 15 types of fragment (i.e., 15 "fragmentation parameters"): The theoretical probability of each data class can then be calculated and compared with its experimental estimate. It is shown below that this overall comparison reveals a significant discrepancy, whose locus is pinned down (to the effects of the sequential position component) by examination of an individual set of symmetries implied by the hypothesis. In both these tests the effect of guessing in the experiment is not taken into account since the phenomenon revealed by this analysis (the malconformation of sequential position) is qualitatively unaffected by the introduction of complicated guessing corrections.

TABLE 3

MAXIMUM-LIKELIHOOD ESTIMATE OF THE PERCENTAGE OF OCCURRENCE OF EACH TYPE OF FRAGMENT

Type	Composition	Occurrence (%)
1	CLOS	9.89
2	CLO	14.36
3	CLS	2.35
4	COS	2.56
5	LOS	4.84
6	CL	.47
7	CO	10.35
8	CS	4.13
9	LO	7.35
10	LS	8.58
11	OS	3.38
12	CL, OS	4.13
13	CO, LS	2.57
14	CS, LO	.70
15	null	24.34

Note. C = color, L = location, O = object type, S = sequential position.

TABLE 4

FRAGMENTATION AND PROPOSITIONAL REPRESENTATIONS OF RECALL FOR EACH CUE TYPE WITH SEQUENTIAL POSITION DELETED AND GUESSING TAKEN INTO ACCOUNT

Pattern no.	Cue type	Correctly recalled	Data frequency	Data (%)	Fragment type(s)[a]	Theoretical (%)	
						Fragmentation	Propositional
1	C	LO	117	24.84	1	25.96	22.72
2	C	L	38	8.07	2	7.50	7.61
3	C	O	77	16.35	3	15.90	15.18
4	C	zero	239	50.74	4, 5	50.63	54.50
5	L	CO	111	23.72	1	25.64	20.75
6	L	C	34	7.26	2	7.82	8.01
7	L	O	62	13.25	4	13.37	12.97
8	L	zero	261	55.77	3, 5	53.17	58.26
9	O	CL	138	29.30	1	26.58	25.52
10	O	C	70	14.86	3	15.29	15.05
11	O	L	57	12.10	4	12.43	12.66
12	O	zero	206	43.74	2, 5	45.70	46.78
13	CL	O	125	46.99	1, 3, 4	48.58	53.40
14	CL	zero	141	53.01	2, 5	51.42	46.60
15	CO	L	93	43.26	1, 2, 4	40.19	48.89
16	CO	zero	122	56.74	3, 5	59.81	51.11
17	LO	C	113	42.97	1, 2, 3	43.04	50.41
18	LO	zero	150	57.03	4, 5	56.96	49.59

Note. C = color, L = location, O = object type.
[a] See Table 5.

Rough estimates of the 15 fragmentation parameters were obtained by simple averaging and algebraic manipulation. They were then adjusted by a computer program using an iterative penalty function technique (see Lootsma, 1972) so as to minimize $2\hat{I}$ for the fit of theory to data, producing the maximum-likelihood estimates shown in Table 3.

These parametric values may be substituted into the fragmentation distributions shown in Table 2 to produce the theoretical percentages shown in the final column. Comparison of the actual and theoretical percentages of Table 2 reveals mostly reasonable agreement, except for the poor fit of the data produced by cuing with sequential position. This impression was confirmed as follows.

First, the information statistic ($2\hat{I}$) for the overall comparison was highly significant at 126.42 (36 degrees of freedom, since there were 50 independent classes of data and 14 free parameters).

Second, a specific symmetry implied by the hypothesis (and one which could be little affected by guessing) was examined. This was the symmetry predicted among the single-cued triple-corrects: that is, $P(\mathrm{LOS}|\mathrm{C}) = P(\mathrm{COS}|\mathrm{L}) = P(\mathrm{CLS}|\mathrm{O}) = P(\mathrm{CLO}|\mathrm{S})$, which arises since each combination can only derive from Fragment Type 1 (i.e., CLOS). It can be seen from Table 2 that the observed ratios of triple-correct to non-triple-correct answers in these four cases were 42/429, 32/436, 53/418, and 86/377, respectively. The deviation from homogeneity is highly significant overall, $2\hat{I}(3) = 34.45$, but it can be attributed entirely to the S-cued ratio. The value of this proportion differs highly significantly from the mean of the other three, $2\hat{I}(1) = 28.84$, whereas these other three do not differ significantly among themselves, $2\hat{I}(2) = 5.61$.

This result suggests that the poor fit overall may simply mean that the model is not applicable to sequential position. The anomalous nature of this component is further investigated later, while first the possibility that the representation of color, location, and object type may be valid is investigated.

Fragments excluding sequential position. The data of Table 2 were aggregated over sequential position to yield Table 4: Cue types which included sequential position were omitted, while sequential position was ignored when occurring as an answer. The data now conform closely to the fragmentation hypothesis.

Since there are only three components instead of four, there are now only five relevant types of fragment possible (see Table 5). Table 4 shows the predicted composition of the data in terms of these fragmentation parameters. In addition, algebraic transformations were applied to the data to correct for the distorting effects of guessing. If $P(n)$ is the probability of occurrence of fragment type n, and g the probability of correctly guessing a particular answer (assumed to be 1/9), it can be seen that (taking as an example the case of cue = color)

$$\begin{aligned} P(\mathrm{LO}|\mathrm{C}) &= P(1) + g[P(2) + P(3)] \\ &\quad + g^2[P(4) + P(5)] \\ P(\mathrm{L}|\mathrm{C}) &= (1 - g)P(2) \\ &\quad + g(1 - g)[P(4) + P(5)] \qquad (2) \\ P(\mathrm{O}|\mathrm{C}) &= (1 - g)P(3) \\ &\quad + g(1 - g)[P(4) + P(5)] \\ P(\mathrm{zero}|\mathrm{C}) &= (1 - g)^2[P(4) + P(5)]. \end{aligned}$$

Maximum-likelihood estimates for each fragmentation parameter, $p(n)$, were obtained as before and are shown in Table 5. When substituted back into the 18 equations of the above type they yield the theoretical representation of the data shown in Table 4.

The comparison of data and theoretical values in Table 4 has 8 degrees of freedom since there are 12 independent classes of data and 4 free parameters. The agreement is close, the difference between data and theory not approaching significance, $2\hat{I}(8) = 4.84$, $p > .75$; in view of the large number of observations involved, the representation is extremely accurate. It is of interest that this goodness of fit does not appear to be strongly dependent upon the precise value of the guessing factor, g, since any discrepancy between data and theory does not reach significance even if guessing is completely ignored (i.e., g set at zero; see Jones, 1974).

TABLE 5

MAXIMUM-LIKELIHOOD ESTIMATE OF THE PERCENTAGE OF OCCURRENCE OF EACH TYPE OF FRAGMENT WITH SEQUENTIAL POSITION DELETED AND GUESSING TAKEN INTO ACCOUNT

Type	Composition	Occurrence (%)
1	CLO	23.83
2	CL	1.32
3	CO	10.77
4	LO	7.56
5	null	56.52

Note. C = color, L = location, O = object type.

TABLE 6

MAXIMUM-LIKELIHOOD ESTIMATES OF PROPOSITIONAL PARAMETERS

Parameter	Estimate
S_{C}	.3143
S_{L}	.2654
S_{O}	.4428
R_{C}	.7485
R_{L}	.6877
R_{O}	.9587

Note. S = stimulus parameter, R = response parameter, C = color, L = location, O = object type.

Propositional Model

Bower (Note 1) has suggested a propositional model, derived from that of Anderson and Bower (1973), for the data of this experiment. Subjects store a composite proposition corresponding to each item, for example, "I saw the yellow cup in the top left location at serial position 2." Each type of component gives rise to a stimulus and a response parameter. These represent, respectively, the probability (S_i) that Component i retrieves the proposition when used as cue and the probability (R_i) that Component i itself is recalled when the proposition has been retrieved by a different component. To allow comparison with the fragmentation hypothesis, the accuracy of the model in representing the data concerning color, location, and object type shown in Table 4 was examined.

Table 6 shows the six parameters which are relevant in this case. These allow theoretical expressions for the probability of occurrence of each of the 18 classes of data of Table 4 to be formulated, for example (Pattern 13) $P(\mathrm{O}|\mathrm{CL}) = [S_{\mathrm{C}} + (1 - S_{\mathrm{C}})S_{\mathrm{L}}]R_{\mathrm{O}}$.

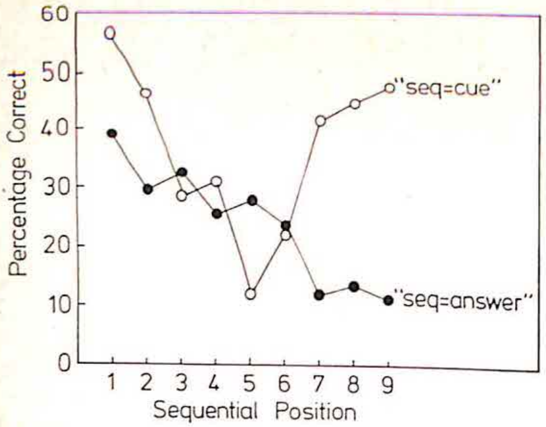

FIGURE 1. Mean percentages of answers correct when sequential position used as the cue ("seq = cue") and when produced as an answer ("seq = answer").

In addition, algebraic transformations were applied to correct for the effects of independent guessing (with probability correct of 1/9) in the same way as before. Table 6 shows maximum-likelihood values of the six parameters, obtained as before, which gave rise to the theoretical representation of the data shown in Table 4. Unlike the fragmentation hypothesis, this propositional model was significantly discrepant with the data, $2\hat{I}(6) = 22.01$, $p < .01$.

Sequential Position

It was shown in the previous sections that whereas the interrelationships of color, location, and object type were in accord with the fragmentation hypothesis, this was not so for sequential position. The analysis below shows that the reason for this is that the accuracy of recall involving sequential position is dependent upon the particular position, and that the form of this functional relationship differs systematically in the three roles that sequential position plays.

First, sequential position may be used to cue for the other components (*seq = cue*). Second, sequential position may be produced as an answer when cued for by another component (*seq = answer*). Third, the accuracy of recall may depend upon the associated value of sequential position even in those cases in which it is not directly involved (*nonseq*). All the analyses described below were conducted on the single-cued data; multiple-cued data are not so useful because only about half of the nine possible sequential positions were involved for each cue type. The 12 different single-cue–recall-component combinations shown in Table 1 may be allotted to the above three classes as follows. First, there are three instances of seq = cue: $P(C|S)$, $P(L|S)$, and $P(O|S)$. Second, there are three instances of seq = answer: $P(S|C)$, $P(S|L)$, and $P(S|O)$. Third, there are six instances of nonseq: $P(L|C)$, $P(O|C)$, $P(C|L)$, $P(O|L)$, $P(C|O)$, and $P(L|O)$.

To summarize the results in advance, they show that in the case of seq = answer, recall resembles psychological judgment concerning an item's sequential position relative to the start of its list. For seq = cue, on the other hand, information concerning the last few items of each list is recalled much more accurately than it is in the case of seq = answer.

Correct recall. Figure 1 shows the percentage of correct recall at each value of sequential position (corresponding to the boldfaced diagonals in Table 7) for seq = cue and seq = answer. Stepwise multiple regression analysis confirmed that these curves were approximately U-shaped and linear, respectively, and that there was no systematic effect of sequential position in the case of nonseq. Similar results were found for the 12 individual cue–recall combinations.

First, for the summed data concerning seq = cue there was no significant linear trend, $F(1, 7) = .03$, but a significant additional quadratic trend, $F(1, 6) = 20.33$, $p < .01$. Similarly, each of the three individual cue–recall combinations did not show a significant linear trend but did show a significant additional quadratic trend ($p < .05$). Second, for the summed data concerning seq = answer, there was a highly significant linear trend, $F(1, 7) = 59.06$, $p < .001$, but no significant additional quadratic trend, $F(1, 6) = .00$. Similarly, two of the three individual cue–

TABLE 7

Distribution of Single-Cued Answers for Each Value of Sequential Position in Three Cases: Sequential Position Used as Cue (seq = cue), Produced as Answer (seq = answer), and Neither Cue Nor Answer (nonseq)

Correct sequential position	Sequential position of answer									
	1	2	3	4	5	6	7	8	9	*Total*
					seq = cue					
1	**90**	24	3	2	16	6	11	2	6	160
2	22	**73**	14	7	8	4	7	11	12	158
3	23	4	**44**	21	20	6	13	15	11	157
4	11	18	7	**49**	30	4	9	7	25	160
5	12	17	11	34	**19**	11	17	24	12	157
6	4	18	18	22	13	**35**	35	8	8	161
7	4	7	8	3	13	23	**66**	16	19	159
8	15	3	20	7	16	16	8	**71**	4	160
9	13	4	11	10	9	7	12	18	**76**	160
					seq = answer					
1	**63**	18	13	17	8	14	20	7	1	161
2	17	**47**	32	32	14	9	9	1	0	161
3	4	17	**51**	29	15	23	12	7	0	158
4	8	18	31	**40**	21	23	14	3	1	159
5	3	10	14	30	**44**	30	17	10	2	160
6	7	13	13	17	22	**37**	22	21	6	158
7	8	10	20	22	28	24	**19**	20	7	158
8	2	12	16	16	25	36	27	**22**	3	159
9	3	11	8	21	20	18	33	29	**18**	161
					nonseq					
1	**137**	25	26	21	23	29	22	22	18	323
2	49	**103**	36	32	25	21	18	21	15	320
3	19	17	**109**	37	14	28	32	27	32	315
4	26	18	29	**142**	21	15	26	14	29	320
5	15	16	24	21	**152**	19	33	20	22	322
6	25	34	21	21	29	**111**	19	27	33	320
7	23	22	38	19	34	21	**115**	31	11	314
8	17	34	26	30	43	26	21	**104**	20	321
9	21	25	20	34	29	29	21	20	**121**	320

Note. The correct sequential position appears in boldface in each case.

recall combinations showed significant linear trends [and the third, from $P(\mathrm{S}|\mathrm{C})$, was almost significant, $F(1, 7) = 5.05$], but none showed a significant additional quadratic trend. Finally, for the summed data concerning nonseq, there was neither a significant linear trend, $F(1, 7) = .31$, nor an additional quadratic trend, $F(1, 6) = .19$. None of the six individual cue-recall combinations of this type showed a significant linear or additional quadratic trend either. Further examination of the summed nonseq data showed that there was nevertheless a significant inhomogeneity, $2\hat{I}(8) = 31.74$, $p < .001$, due perhaps to the probabilities of formation of different types of fragment not being independent of the particular nonseq component values concerned.

Distribution of errors. Each row of Table 7 shows the distribution of single-cued answers about a particular correct (boldfaced) value of sequential position. For seq = answer, each row shows the number of times each value of sequential position occurred as the answer; for example, the first line of the seq = answer data of Table 7 shows the distribution of the 161 answers when the correct answer was 1 (the three relevant cue-recall types occurred once each for each of the 54 subjects, with one answer missing). For seq = cue and nonseq, each C, L, or O value corresponded to one of the nine items in the current presentation set (since all nine values of each component were used in each set) and is allotted in Table 7 to that item's corresponding sequential position.

Inspection of Table 7 shows that whereas for nonseq the distribution of errors over sequential position appears random, for seq = cue and seq = answer errors tend to be near-misses, as though sequential position were acting as a psychophysical dimension. In accordance with this hypothesis, analysis showed (a) that overall the distributions over the off-diagonal cells in each row were not uniform, $2\hat{I}(189) = 848.58$, $p < .001$, and (b) that the distributions over the off-diagonal cells of the three matrices, compared row by row, differed significantly, $2\hat{I}(126) = 515.25$, $p < .001$.

The nonseq data shown in Table 7 cannot be further examined to determine whether near-miss errors also occurred for the color, location, and object-type components because of the absence of a measure of stimulus proximity in these cases. An exception occurs for location, since a plausible hypothesis is that errors tended to be concentrated in the four locations in either the same row or the same column of the 3×3 stimulus matrix as the correct value, rather than in the other four error locations. Analysis showed that this disproportion just reached significance in the case of location = answer, for which 54.1% (324/599) of errors occurred in the correct row or column, $2\hat{I}(1) = 4.01$, $p < .05$, but failed to do so in the case of location = cue, for which 53.3% (335/628) of errors corresponded to these locations, $2\hat{I}(1) = 2.81$. Thus, for location at least, the incidence of near-miss errors was rather small. These errors possibly arose from confusions in recall, but could also have resulted from occasional encoding of only the horizontal or vertical locations of an object, instead of their conjunction. In the case of the different values of a component being easily confused perceptually (as for sequential position in this experiment), it is likely that the fragmentation hypothesis does not generally hold, since although overall recall might still be symmetrical, retrieval is likely to be probabilistic in nature.

Representation in memory. The results concerning sequential position suggest that it is represented in memory in terms of an item's apparent displacement from either the beginning or the end of its sequence. Such a displacement might be expected to be of a similar form to that arising in the psychophysical judgment of duration (Stevens, 1957). Since this dimension is prothetic, the subjective magnitude, $J(n)$, of the displacement of sequential position n from the start should be given by a power function, $J(n) = An^B$ or $\log J(n) = B \log n + \log A$.

This relationship was tested by examining the variation of the average judgment of sequential position (i.e., seq = answer) with different (correct) values of sequential position. Because of the skewness of the error distributions, medians were used as the best indicators of centrality.

Figure 2 indicates that the median value in seq = answer was increasingly underestimated at higher values; indeed for Sequential Positions 7, 8, and 9 Table 7 shows that the modal answers corresponded to Positions 5, 6, and 7, respectively. It was shown that Figure 2 depicted an approximately power function relationship by transforming the axes logarithmically. The resulting best-fitting straight line was $\log J(n) = .442 \log n + .371$; the linear correlation was .983, accounting for 96.6% of the variance. The value of the power-law exponent here, .442, is considerably less than the value of 1.05 – 1.2 obtained for the direct estimation of temporal duration (Stevens, 1957). This may have been because the limitation of the response set to the first nine ordinal numerals made the task resemble category judgment rather than magnitude estimation (see Stevens, 1971; Stevens & Galanter, 1957).

Figure 2 also shows the median sequential position associated with the answers at each value of sequential position when this was used as the cue (i.e., seq = cue). It can be seen that this curve differs from the seq = answer curve in being approximately symmetric about its center. Post hoc, this difference and that of Figure 1 may perhaps be attributed to subjects employing information relating only to the start of the list for seq = answer, but to both the start and the end of the list for seq = cue.

Performance in Successive Lists

The average level of performance fell steadily over the nine lists of each session, the mean level of correct recall in the ninth set being only 67.1% of that in the first set. It is shown below that this decrement was confined to the data not involving sequential position and could not be accounted for by overt intrusion errors from previous lists.

The mean level of performance for single-cued recall mirrored overall performance in dropping to 67.2% of its initial level by the ninth set. Of the constituent 12 single-cue–answer-component combinations, however, the impairment was confined to the six nonseq types, recall here dropping in the ninth set to 55.9% of its initial level. For the other six types (i.e., seq = cue and seq = answer) mean recall in the ninth set was 84.0% of that in the first set, regression analysis of mean performance over the nine sets indicating no significant linear trend, $F(1, 7) = 3.15$, or additional quadratic trend, $F(1, 6) = 3.87$. These results mean, of course, that the values of the fragmentation parameters shown in Tables 3 and 5 represent mean probabilities of formation of the different types of fragment, averaged over different sets as well as different stimuli.

Investigation of the nature of the errors (see Jones, 1974) showed that the increase in their frequency during the course of the session did not correspond to any rise in the very low rate of intrusion of combinations of values which occurred in stimuli of previous sets. It is a logical possibility, however, that the apparently random errors in fact represented the intrusion of single values from previous lists.

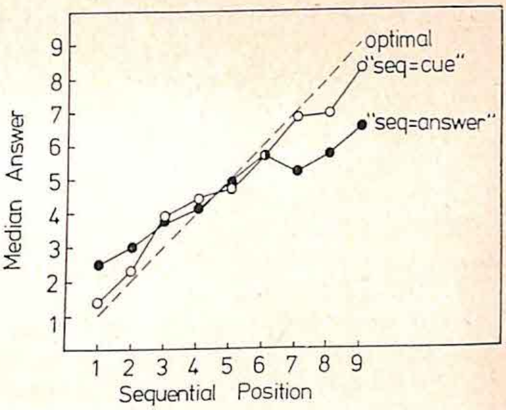

FIGURE 2. Median answer values when sequential position used as the cue ("seq = cue") and when produced as an answer ("seq = answer"). (The dotted line represents median values equal to the corresponding sequential positions.)

The ability of subjects to restrict recall to the most recent list, even though lists were composed of differing combinations of the same elements, must involve the use of information other than that directly investigated here; for example, an accessed item might be suppressed if it has a list contextual component or "list marker" (Anderson & Bower, 1972) which upon retrieval is found to be incorrect. The contrast between the decline in the level of nonseq recall over each session and the stationarity of recall involving sequential position was unexpected. Two untested possibilities are that the difference arose either because an improving accuracy of estimation of sequential position counteracted a general proactive decrement or because the registration of the visual components, unlike that of sequential position, was initially enhanced as a result of their novelty.

DISCUSSION

Fragmentation

A hypothesis has been proposed which yields an accurate representation of patterns of recall of color, location, and object type (but not of sequential position) obtained during single and multiple cuing. The memory trace derived from a particular event corresponds to one or more *fragments* of the event, each containing a nonoverlapping subset of the components representing that event. The provision of a single one of these components as a cue renders the fragment accessible to recall in an all-or-none way. Three corollaries follow.

Symmetry. First, the hypothesis correctly implies that certain specific symmetries should exist among the frequencies of certain patterns of recall to individual cuings: for example, that $P(\mathrm{LO}|\mathrm{C}) = P(\mathrm{CO}|\mathrm{L}) = P(\mathrm{CL}|\mathrm{O})$.

It was noted also that the *overall* single-cued conditional probabilities were reflexive for color, location, and object type: for instance, overall $P(\mathrm{L}|\mathrm{C}) = P(\mathrm{C}|\mathrm{L})$. This in fact is a general prediction of the model, since the recall of L given C can occur only if the relevant fragment contains both L and C, in which case cuing with L would also produce C.

Note however that this reflexivity does not hold for the overall *multiple*-cued probabilities. This is readily seen by considering the effect of double cuing (with C and L) the following two types of fragment. For a CLO fragment, it is true that cuing with CL will produce recall of O, CO will produce L, and LO will produce C. However, recall of an LO fragment is asymmetric, since CL will again produce O and CO will produce L, but cuing with LO will be unsuccessful. Thus reflexivity of overall conditional relationships is to be expected only in the case of single cuing.

Multiple cuing. The hypothesis states that for an individual engram the use as cue of only one of its components is necessary for recall to occur. Consequently, additional cuing of that fragment is redundant. However, summed over a range of fragments we may expect multiple cuing to be numerically more effective than single cuing. This is because the "first" cue may not be one of the stored components comprising the relevant fragment and would therefore have no effect. Consequently, the "second" cue would effectively increase the probability of recall from zero to one.

As an example, consider the following two instances of double cuing. First, suppose that a particular set of fragments are all of the CLO type. Then it can be seen that double cuing using both L and O will here be completely redundant and $P(\mathrm{C}|\mathrm{LO}) = P(\mathrm{C}|\mathrm{L}) = P(\mathrm{C}|\mathrm{O})$. Second, suppose that the set of fragments are of the two different types, CL and CO. Then in this case $P(\mathrm{C}|\mathrm{LO}) = P(\mathrm{C}|\mathrm{L}) + P(\mathrm{C}|\mathrm{O})$. Hence it can be seen that the general bounds within which the fragmentation hypothesis allows the overall double-cuing probabilities to lie are quite wide:

$$\text{maximum}[P(a|b), P(a|c)] \leqslant P(a|bc) \leqslant P(a|b) + P(a|c). \quad (3)$$

In particular, the double-cued probabilities allowed by Equation 3 can be either greater or less than those expected on the assumption that cues act by exciting associations independently:

$$P(a|bc) = 1 - [1 - P(a|b)][1 - P(a|c)]. \quad (4)$$

Thus the fact that in the present experiment the effect of additional cuing was relatively small sufficed to rule out the application of the independent cue Equation (4) but was interpreted in fragmentation terms as a large redundancy effect: the occurrence of many fragments containing more than just two components.

Foss and Harwood (1975) have proposed that, contrary to Equation 3, a double-cued probability of recall may exceed the sum of the corresponding single-cued probabilities, at least for the recall of sentences. They suggest that this could be due to the encoding of configural or Gestalt information concerning an item. In fragmentation terms this could be represented as a fragment component which in retrieval can be correctly distinguished from others only by its correspondence to the conjunction of two or more stimulus components, not to these individually. In the same way, in the present experiment a specified location value could not have been successfully distinguished from other values by the provision of either its horizontal or vertical coordinate alone. It is not clear at the moment whether such encoding of sentences does occur to a significant extent, since Foss and Harwood (1975, p. 10) found that overall the effect did not reach conventional levels of significance.

All-or-none processing. A third corollary of the hypothesis, and perhaps its strongest prediction, is that the recall of individual memories is an all-or-none process. In other words, it postulates that even though processing of the overt, whole, item to be remembered may not be all or none, that of the underlying trace is. The hypothesis implies, in fact, that an *overall* all-or-none pattern of recall is likely to be found only with simple binary material, which is in accord with previous results (see Bower, 1967a).

Propositional Model

An alternative model for the data of this experiment has been proposed by Bower (Note 1). This postulates that each item is stored as a composite proposition. Each component has a particular probability of retrieving the proposition, and another of being itself recalled subsequent to retrieval. The model differs from that advanced by Anderson and Bower (1973, chap. 10) because that model, in addition to postulating stochastic input and output, centered upon the prediction of the relative likelihood of encoding of each part of the proposition. Such prediction would not be possible here. This is because the prediction is based upon the structure allotted to the input by a parser, and the perceptual parser of Anderson and Bower's computer program HAM, unlike its linguistic parser, has not been specified.

The fragmentation hypothesis and Bower's propositional model are antithetical in their characterizations of the recall process, the former postulating that it is fundamentally all or none in operation, the latter that it is in essence probabilistic. In an empirical comparison it was found that the fragmentation hypothesis provided the more accurate representation, even though it employed two fewer free parameters.

Sequential Position

Memory for sequential position did not accord with the fragmentation hypothesis. This was because the efficacy of recall involving sequential position varied with sequential position in two different ways, depending on whether sequential position was being used as the cue (seq = cue, U-shaped function) or was itself produced as the answer (seq = answer, linearly decreasing function). In the latter case, the median recalled value of sequential position was related to the true value by a power function, similar to that arising in the psychophysical judgment of duration (Stevens, 1971). The asymmetry of processing was interpreted, post hoc, as being due to the use of information relating either to both ends of the list (seq = cue) or to the start of the list only (seq = answer). It is, in addition, theoretically possible (though in this case untested) that the fragmentation hypothesis also fails for sequential position because it interacts in an anomalous way when combined with other attributes as a multiple cue.

The existence of a recency effect (i.e., enhanced recall of later items in a series) in the seq = cue condition constitutes a problem for many explanations of the effect. This is because these explanations attribute the recency effect observed in, for example, free recall (Murdock, 1962) to the immediacy of recall after presentation. In the present experiment, however, there was a 25-sec period of intervening activity, which ought to have destroyed the recency effect by displacing the last items from their place in the short-term store (Atkinson & Shiffrin, 1968; Glanzer, 1972; Waugh & Norman, 1965) or by interfering (retroactively) with these items in just the same way that they themselves had interfered with earlier items in the list (Bernbach, 1969).

The survival of the recency effect was interpreted here as arising from the use of knowledge of sequential position which related to the end as well as the start of each list; in its assumption of bipolarity the explanation is related to those of Feigenbaum and Simon (1962), Murdock (1960), and Bower (1971). It is, moreover, plausible that in free recall implicit analogues of sequential position are used as cues for recall and that it is the psychophysical nature of these which gives rise to observed recency effects. Temporal proximity might be expected to be an important implicit attribute and would decrease in efficacy with increasing delay between presentation and recall, as suggested previously by Bjork and Whitten (1974).

In conclusion, the fragmentation hypothesis provides not only an accurate representation of major features of the recall of fairly complex and reasonably naturalistic events, but also a framework within which to identify exceptional phenomena. Further work (Jones, 1974) has shown that the hypothesis also provides an accurate representation of the results of experiments investigating verbal memory, including those of Anderson and Bower (1973, chap. 10).

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ERRATUM TO MONTI AND SMITH

In the article "Residual Fear of the Conditioned Stimulus as a Function of Response Prevention after Avoidance or Classical Defensive Conditioning in the Rat" by Peter M. Monti and Nelson F. Smith (*Journal of Experimental Psychology: General*, 1976, Vol. 105, No. 2, pp. 148–162), the information presented in the right-hand column of page 160, lines 19–38, was incorrect. The sentences should read as follows: The first was a three-way repeated measures analysis of variance comparing A-B, CD-B, A-NBHC, and CD-NBHC on Trials 1–3. The results indicated that A-B had significantly more response suppression than CD-B, $F(1, 36) = 4.48$, $p < .05$; and A-NBHC had significantly more response suppression than CD-NBHC, $F(1, 36) = 19.91$, $p < .01$. The interaction was not significant. The second analysis was a three-way repeated measures analysis of variance comparing A-B, CD-B, A-NBHC, and CD-NBHC on Trials 10–12. The results indicated that A-B had significantly more response suppression than CD-B, $F(1, 36) = 4.33$, $p < .05$. No differences were found between the nonblocked conditions, and the interaction was not significant.

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Frequency Analysis of Attribute Identification

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SUMMARY

The frequency with which values on any stimulus dimension occur within the positive (and/or negative) category of a concept is a potential cue to which dimensions are relevant. One might theorize that the development of frequency differentials among stimulus values over a series of encounters with positive and negative instances is the way people identify relevant attributes of a concept. Suggestive evidence on the importance of frequency in this process comes from our observation, made in an already published experiment (Bourne & Guy, 1968), that a series of instances which affords no large differentials is almost impossible to solve. Seven experiments, using a two-stimulus (one positive and one negative) simultaneous presentation paradigm, designed to explore frequency effects systematically are reported in this article. In the first, we show that the order of difficulty of four conceptual rules—conjunctive, disjunctive, conditional, and biconditional—in the attribute identification task is correlated with the magnitude of the frequency differentials these rules generate in a stimulus sequence. In two subsequent studies, we show that a difficult rule, the biconditional, can be made more or less difficult (indeed, quite simple) by artificially manipulating value frequencies. The frequency cue of greatest importance exists when a relevant value, a, appears more often than do all irrelevant values, $\bar{a}$, on the same dimension within the positive category of instances. The importance of $a > \bar{a}$ is not affected by reinstating either the positive or the negative stimulus at the time of feedback, suggesting that the subject codes both stimuli at their initial presentation in each trial and augments the frequencies of values appropriately once the positive stimulus is identified by feedback. The remaining studies indicate certain qualifications on these conclusions. First, the reintroduction of an instance (positive or negative) at feedback probably adds partial increments to frequency counts serving to augment their effectiveness as cues to the relevant values. Second, differentials within the negative category can be and are used as cues to problem solution when no other cues are available. The effects observed in these studies are fully in accord with an extension of the frequency theory of discrimination learning (Ekstrand, Wallace, & Underwood, 1966). This extension accounts for a number of other results in the literature, such as the effects of intradimensional variability on attribute identification, and leads to certain expectations about stimulus sequence effects and the abstraction of prototypes representative of natural categories.

In an attribute identification problem, the subject must determine which one or more values of several variable stimulus dimensions is (are) relevant to an unknown concept and must define the positive category of stimuli. These processes are accomplished by using information provided by a series or set of stimuli identified by feedback as positive or negative category members. It has been clear for some time that positive and negative instances are differentially informative to a subject regarding the concept. The early literature suggested that positive instances or a mixture of positive and negative instances are generally more useful than negative instances alone (e.g., Hovland & Weiss, 1953). It has since become clear that the more useful type of instance depends on the nature of the concept to be determined (Bourne & Guy, 1968). When the concept is a simple affirmation of a single stimulus value or a conjunctive combination of two or more values (the typical type of concept used in earlier work), positive instances are indeed more effective in delineating the concept. If, however, the concept is a conditional relationship among values, negative instances appear to be more informative. In other cases, for example, a disjunction of values, there is little difference in the usefulness of positive and negative instances.

These empirical results are open to a variety of interpretations, most of which emphasize the difference in informational content between positive and negative instances. There are methods for equating information across instances, however, and performance differences obtain even in these cases (Bourne & Guy, 1968; Hovland, 1952). Furthermore, none of these interpretations offers much insight into the processes that underlie the use of informational differences.

There is a feature of concept stimuli which is so simple and so obvious that it has virtually escaped any notice. This is a feature known to be of critical importance in other, simpler discrimination tasks (Ekstrand, Wallace, & Underwood, 1966), namely, the frequency with which the values of the relevant stimulus dimension(s) occur within concept categories relative to values on irrelevant dimensions. Value frequency tends to correlate with informational content of stimuli under natural conditions, though it can be manipulated independently. The frequency differentials that exist within and between categories could be of considerable use to a subject as he attempts to identify the relevant stimulus value or combination of values.

There are theories (Ekstrand et al., 1966) that would lead us to expect a frequency effect. In general, the frequency theory states that subjects keep some kind of record of frequency differentials among stimuli they counter. Frequency information can then be used by the subject as a guide to correct performance in a discrimination task. In verbal discrimination learning, the theory postulates that, across trials, the correct item in each pair "occurs" more frequently than the incorrect item. The subject can perform this task by selecting the item that has a higher (or, on occasion, lower) frequency count. Applied to attribute identification, the frequency theory stipulates that subjects will keep some record of the differing frequencies of the various stimulus values within positive

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and/or negative instances of the concept. It is the case that, for all conceptual rules, the relevant values are distinguishable from the irrelevant ones simply on the basis of differential frequency. By merely "keeping track" of the frequencies of all the values in positive or negative instances, the subject can discover the relevant ones. In short, the only information the subject needs to solve the typical attribute identification problem, in which the rule frame is known in advance, is the (approximate) frequency of each stimulus value in positive and/or negative instances.

Consider, for example, the case where the concept is defined by the conjunction of redness and squareness. Suppose the population of instances is defined by four dimensions and there are three values on each dimension, yielding a total set of possible instances numbering 81. The dimensions are color (red, yellow, or green), shape (square, circle, or triangle), number (1, 2, or 3), and size (small, medium, or large). The conjunction of red and square means that only patterns that are *both* red and square will be positive instances of the concept. In this population, there are nine such patterns, and if we count the number of times each of the values occurs in these nine patterns, we come up with the following results: red–9, yellow–0, green–0; square–9, circle–0, triangle–0; one–3, two–3, three–3; small–3, medium–3, large–3. It can be readily seen from these counts that differential frequencies occur only on the two relevant dimensions (color and shape). Given this set of counts, it is immediately possible to identify the relevant dimensions as color and shape and to determine that red and square are the relevant values.

The frequency theory of attribute identification thus postulates that subjects, in the course of examining patterns from the stimulus population, become aware of frequency differentials among the values, which in turn allows them to determine the relevant ones. It follows, therefore, that the basic variable determining the difficulty of any particular attribute identification problem will be the degree to which frequencies differ across and within dimensions in a stimulus sample of fixed size. When frequencies differ widely, the problem will be easy to solve (as in the above example). Other rules or other manipulations might result in frequency differences that are smaller and more difficult to detect, and this would serve to make the problem more difficult. The basic cue consists of a frequency differential that identifies the relevant values, and the difficulty of the problem will be determined by the difficulty the subject has in discovering that a differential exists.

We were alerted to the possible importance of frequency in attribute identification by a surprising outcome in the experiment reported by Bourne and Guy (1968). In that study, concepts were based on three of the primary bidimensional rules (Bourne, 1970), conjunctive, disjunctive, and conditional rules. When subjects were presented with only positive instances in an attribute identification task, the conjunctive rule was by far the easiest, as has typically been found. When subjects were presented with only negative instances, however, the conjunctive rule became the *most difficult* of all rules used. Indeed, only 2 of the 12 subjects who participated were able to solve a conjunctive problem on the basis of negative instances alone, even after 96 successive study trials. Given the procedure used, this is a very difficult concept indeed.

We present, in Table 1, mean trials to solution for certain conditions of the Bourne and Guy (1968) experiment. These data are based on test trials only, of which there was a total of 48 intermixed among study trials. We also show in Table 1, for all primary bidimensional rules, the percentages of both positive and negative instances, presented on study trials, which exhibited a relevant stimulus value. These percentages are determined by the stimulus-to-category assignments specified by each rule used in that study. For example, positive instances of a conjunction always exhibit both relevant values. Negative instances, however, show little differential among the (three) values on either relevant dimension. Thus, one reason why negative instances may be so difficult (perhaps

TABLE 1

PERCENTAGES OF OCCURRENCES OF VALUES ON RELEVANT DIMENSIONS IN CONCEPT CATEGORIES

Dimension and value	Rule: Conjunctive, Positive category	Conjunctive, Negative category	Disjunctive, Positive category	Disjunctive, Negative category	Conditional, Positive category	Conditional, Negative category	Biconditional, Positive category	Biconditional, Negative category
Dimension A								
a	100	33	67	0	33	100	50	50
$\bar{a}$[a]	0	33	16	50	33	0	25	25
Dimension X								
x	100	33	67	0	67	0	50	50
$\bar{x}$[a]	0	33	16	50	16	50	25	25
Mean trials to solution	3.92	46.3	12.9	19.8	28.8	16.9	—[b]	—[b]

Note. Adapted from Bourne and Guy (1968).

[a] There are two $\bar{a}$ and $\bar{x}$ values. For example, if the A dimension is color, a might be red and $\bar{a}$ green and blue. Some percentage columns sum to less than 100 because of the necessity to double $\bar{a}$ and $\bar{x}$ values. Irrelevant dimensions are not represented. All values from an irrelevant dimension occur 1/3 of the time (33%) in both positive and negative categories.

[b] The biconditional rule was not examined in this study.

impossible) to work with is that they provide no frequency discrimination among the values on relevant or irrelevant dimensions.

Results obtained with disjunctive and conditional concepts are also consistent with the notion that frequency differential is one possible reason for the difference in effectiveness between positive and negative instances. The negative instances of a conditional show larger frequency differentials, and subjects do better with them than with positives. This is an interesting case in view of the frequency relationships that exist. Within the negative category, one relevant value, call it *a* from Dimension A, has greater frequency than both irrelevant values from the same dimension, $\bar{a}$. The other relevant value, *x* from Dimension X, has lesser frequency than $\bar{x}$. It would appear as though the subject can use either differential, favoring *a* or $\bar{a}$, in deciding how to respond. It is not clear in these data whether one is easier to use than the other. Note, however, that the only difference between disjunctive and conditional concepts in the negative instance condition lies in the frequency differential which applies to Dimension A. The fact that conditional–negative problems are slightly easier to solve than disjunctive–negative problems might be taken to suggest that a differential favoring the relevant value (*a*) might be more effective than a differential favoring irrelevant values in the same dimension, although there are too few data to be sure.

Positive instances of a conditional provide a frequency differential only on one of the two relevant values. This clearly contributes to the difficulty of conditional concepts when the subject has only positive or a mixture of positive and negative instances to work with. It suggests, further, that one of the two relevant values ought to be easier to identify than the other. Examination of the trial-by-trial data in this condition indicates that 9 of 12 subjects did identify the relevant dimension with a frequency differential first in conditional problems; for the remaining 3 subjects there was no difference between the two relevant dimensions.

Frequency differentials may help to explain differences in difficulty among conceptual rules. Commonly, it has been found that the four primary bidimensional rules—conjunctive, disjunctive, conditional, and biconditional—differ significantly in the order given. While there are no data in the Bourne and Guy (1968) experiment on biconditional concepts, the arrangement of frequency differentials appearing in Table 1 does suggest reasonable correspondence with the typically observed difficulty ordering (Bourne, 1970).

Frequency differentials were not manipulated in the experiment by Bourne and Guy (1968), nor was the study designed to evaluate which, if any, frequency cues a subject might use in an attribute identification problem. The possibility of a frequency explanation arose after the fact, upon observing performance outcomes in various conditions. The results are suggestive, however, and merit further investigation. It is to that end that the following series of experiments was conducted.

Frequency Differentials

There are several ways in which the frequency of events over a series of trials in an attribute identification problem can distinguish the relevant stimulus values of a concept from irrelevant values. All values from irrelevant dimensions occur equally often in the positive and negative categories in our experiments. Thus, there is never any frequency distinction among values of an irrelevant dimension in either concept category. Differentials occur within categories only among values on the relevant dimensions (and between relevant and irrelevant dimensions). A priori, we can list these differentials and then empirically ask which of them appears to be associated with problem-solving performance.

There are two relevant values, one from each of two relevant dimensions, in each of the problems to be considered. Call the relevant values a and x. Other irrelevant values on the same relevant dimensions, A and X, will be referred to as $\bar{a}$ and $\bar{x}$. Thus, if Dimension A was color, a might be red and $\bar{a}$ yellow and green. In most cases, frequency differentials which apply to A will, likewise, apply to X. Only the conditional rule contains an asymmetry such that the frequency differentials are different for A and X.

Within-category differentials. Values on a relevant dimension differ in frequency of occurrence within a category. These differences may be such that defining value a occurs either more or less frequently than $\bar{a}$. Consider, for example, the positive category of a conjunction and the negative category of a disjunction. As Table 1 shows, a occurs 100% and $\bar{a}$ 0% of the time in the first case, while each $\bar{a}$ (of which there are two) occurs 50% and a 0% of the time in the second case.

Between-category differentials. Particular values on a relevant dimension differ in frequency of occurrence between categories. These differences may be such that any value, a, occurs more or less frequently in the positive category than in the negative category. For example, value a occurs on 100% of positive instances and only 33% on negative instances of a conjunctive concept. In contrast, a occurs on 33% of positive instances and on 100% of negative instances of a conditional, as shown in Table 1.

EXPERIMENT 1

A two-stimulus, simultaneous discrimination procedure has been used in most investigations of frequency effects and tests of the frequency theory interpretation of verbal discrimination learning. With a few significant exceptions (e.g., Levine, 1967; Wells, 1967), this procedure is not characteristic of research on concept learning. The first experiment was executed to determine whether rule effects typically observed in concept problems could be produced with the simultaneous discrimination paradigm. Thus, on each trial of their problem, subjects were presented with two stimuli, one of which was positive and the other negative. Their task was to pick the positive instance. Different groups of subjects were required to solve a concept problem based on each of the four primary bidimensional rules, conjunctive, disjunctive, conditional, and biconditional. If this paradigm requires the usual concept learning processes, we should expect the same ordering of rule difficulty as is obtained under other experimental procedures, namely, the order in which rules are listed above. A further point of interest lies in possible frequency effects. As noted, different rules generate different frequency relationships among the values on the two dimensions which are relevant to the solution of the problem. Thus, the results afford an opportunity to examine, once again, the global effects of frequency cues and their possible contribution to rule difficulty.

Method

Subjects. The subjects were 96 college students, 48 receiving credit in an introductory psychology course for their participation and the other 48 receiving $2 in cash payment.

Experimental design. The design was a 4 × 2 × 2 factorial, including four logical rules (conjunctive, disjunctive, conditional, and biconditional), two stimulus sequences, derived from different stimulus populations, and two problems, differing in the pair of values chosen to be relevant (1–yellow and small–square).

The stimulus population consisted of 81 geometric designs, varying in four dimensions. Stimulus sequences were selected according to natural or truth-table principles. When sequences are formed according to the natural principle, each stimulus has an equal chance of being selected on any trial, subject to the constraint that one stimulus must be positive and the other negative to comprise a pair. Of the total of 81 stimuli, 9 are positive instances and 72 are negative instances of a conjunction. Thus, each trial presented the subject with one randomly chosen instance from the set of 9 positives and a companion randomly chosen instance from the set of 72 negatives. Similar arrangements apply for the three other concept types. Following the truth-table principle, all truth-table combinations have an equal probability of being represented by the stimuli on each trial, subject to the constraint that one stimulus must be positive and the other negative. In other words, stimuli with both (TT), the first but not the second (TF), the second but not the first (FT), and neither (FF) relevant attribute have an equal prob-

TABLE 2

PERCENTAGES OF OCCURRENCES OF VALUES ON RELEVANT DIMENSIONS IN POSITIVE AND NEGATIVE CATEGORIES IN EXPERIMENT 1

Dimension and value	Rule							
	Conjunctive		Disjunctive		Conditional		Biconditional	
	Positive category	Negative category	Positive category	Negative category	Positive category	Negative category	Positive category	Negative category
Natural population								
Dimension A								
a	100	25	60	0	15	100	20	50
$\bar{a}$	0	37.5	20	50	42.5	0	40	25
Dimension X								
x	100	25	60	0	42.5	0	20	50
$\bar{x}$	0	37.5	20	50	28.8	50	40	25
Mean trials to solution	6.0		30.0		41.0		56.0	
Mean errors to solution	2.1		11.8		16.5		23.8	
Truth-table population								
Dimension A								
a	100	33	70	0	33	100	50	50
$\bar{a}$	0	33	15	50	33	0	25	25
Dimension X								
x	100	33	70	0	65	0	50	50
$\bar{x}$	0	33	15	50	17.5	50	25	25
Mean trials to solution	6.0		28.0		39.0		54.0	
Mean errors to solution	2.0		12.5		17.3		26.8	

Note. a and x = relevant values, $\bar{a}$ and $\bar{x}$ = irrelevant values.

ability of being selected on any trial. For a conjunction, the positive stimulus on each trial would be chosen from the TT class. Two steps were required to obtain a negative stimulus. First, one of the remaining three classes (TF, FT, or FF) was randomly chosen; then a stimulus within that class was selected. The stimulus sequences used in this and all remaining experiments provided sufficient information for a subject with perfect memory making errorless inferences to identify the relevant stimulus values usually in two and, at most, in three trials.

The stimuli were geometrical designs, having the following dimensions (and levels): color (red, yellow, or green), number of figures (1, 2, or 3 identical figures), size (large, medium, or small), and shape (square, triangle, or circle). Table 2 shows the relative frequency (percentage) of occurrence of values from the two relevant dimensions for the various rules and for both natural and truth-table stimulus sequences.

Subjects were fully instructed as to the dimensions and the values of the stimuli and the rule which defined solution to their problem. They were given a number of examples of how the rule for their problem could be used to combine two stimulus values to form a positive and a negative category. Subjects demonstrated their knowledge of the rule by categorizing stimuli according to a stated solution given by the experimenter. Furthermore, subjects were asked to describe partitionings of the stimulus population using the rule and two values named by the experimenter. In the experimental problem, each trial confronted subjects with two stimuli, one positive and one negative, presented with two slide projectors on a translucent viewing screen. The right–left position of stimuli varied randomly. Each trial consisted of a subject-paced response interval, a 5-sec feedback interval during which only the positive slide was available for the subjects' inspection, and a 2-sec intertrial interval. The subjects' task was to pick the positive stimulus on every trial. Subjects knew the rule for solution, but had to determine the two relevant attributes in order to be able to pick the positive stimulus. Trials continued until subjects met a criterion of 16 correct responses or reached Trial 80 without solution.

Results and Discussion

Table 2 shows, for each of the eight main conditions in this experiment, the proportion of time a, $\bar{a}$, x, and $\bar{x}$ occur in the positive and negative category of a given concept problem. Also shown in Table 2 are the mean trials and mean errors to the solution for each problem. Performance in trials differs reliably among conceptual rules, $F(3, 80) = 33.48$, $MS_e = 328.68$. In this and all other statistical tests $p < .05$ was considered reliable. There were no main effects or interactions involving stimulus population or problems. Similar results obtained in an analysis of errors The rule order found in Experiment 1 with the discrimination paradigm is the same as that obtained in both the standard anticipation procedure (Haygood & Bourne, 1965), the study test procedure (Bourne & Guy, 1968), and usually (Bourne, Ekstrand, & Montgomery, 1969), but not always (Laughlin, 1968), the selection paradigm. Thus, these results indicate that performance can be expected to be essentially the same over significant changes of the experimental paradigm.

The results are suggestive, though not definitive, with respect to the importance of various frequency differentials which apply to values on the relevant dimensions. Perhaps the major outcome is that differentials within the positive category are clearly more effective than are those within the negative category. Note that both the disjunctive and the conditional rule show large differentials within the negative category, and yet are considerably more difficult than conjunctives which show only a small difference within the negative category. In contrast, the conjunction, which shows the greatest differential within the positive category, is by far the easiest problem to solve. The biconditional, with weak differentials in both categories, is, as might be expected, the most difficult problem to solve.

It appears as though subjects focused primarily on the positive category, hardly surprising in view of the results of earlier experiments (Levine, 1967). Moreover, the reader will recall that the positive instance was retained after subjects responded as a form of feedback. Subjects had an opportunity to examine the positive instance on each trial for some period of time after they found out whether they were right or wrong, which was not the case for the negative instance. We shall have more to say about this arrangement later. Differentials within the negative category suffer from another potential disadvantage. For the disjunction, $\bar{a}$ and $\bar{x}$ occur exclusively, and a and x never appear in the negative category. From earlier evidence (Bourne & Guy, 1968), it is likely that subjects cannot use cue differentials in which $\bar{a}$ exceeds a as effectively as a exceeds $\bar{a}$. The evidence is not completely clear, however, and we shall examine this question more thoroughly in Experiment 2, and elsewhere.

It seems likely from these data that subjects rely primarily upon within-category frequency differentials. Furthermore, the tendency to use these differentials appears to be limited to the positive category. This experiment was not designed, however, to manipulate within- and between-category differentials systematically. Rather, the intent was to examine a paradigm, like the procedure used in other experimental evaluations of frequency cues, to discover whether rule differences similar to those obtained in other circumstances still exist, and tentatively to judge whether or not frequency differentials are related to performance on different conceptual rules. In subsequent experiments, we examine more rigorously the effect of different types of frequency differentials within a single problem type.

EXPERIMENT 2

We have established that rule differences appear in a simultaneous discrimination version of the attribute identification task, just as they do under the standard anticipation, the selection, and the study–test procedures. Further, we have shown that frequency differentials, especially those within the positive category, correlate with obtained rule differences. Finally, there is a suggestion in these results that the occurrence of a (or x) with greater frequency than $\bar{a}$ (or $\bar{x}$), especially in the positive category, is the most effective cue for attribute identification. The present study was designed to examine two of these issues more systematically. Our approach was to select a single conceptual rule and manipulate frequency differentials within that rule. To insure the maximal opportunity for effect, the most difficult of the primary bidimensional rules, the biconditional, was chosen. All frequency manipulations were made within the positive category, the negative (by necessity) being held constant across conditions. Within the positive category, we can arrange stimulus sequences such that a and x are not distinguished, frequency-wise, from $\bar{a}$ and $\bar{x}$, conditions where a and x are more frequent within the positive category than $\bar{a}$ and $\bar{x}$, and conditions in which a and x are less frequent than $\bar{a}$ and $\bar{x}$ in the positive category. Further, we can manipulate the absolute magnitude of frequency differentials between a (or x) and $\bar{a}$ (or $\bar{x}$). Five frequency relationships were employed in this experiment, as shown in Table 3.

Method

Subjects. The subjects were 60 students in introductory psychology classes who participated to fulfill a course requirement. None had previous experience in an attribute identification experiment.

TABLE 3

PERCENTAGES OF OCCURRENCES OF VALUES ON RELEVANT DIMENSIONS IN CONCEPT CATEGORIES IN EXPERIMENT 2

	Frequency condition									
	1		2		3		4		5	
Value	Positive category	Negative category	Positive category	Negative category	Positive category	Negative category	Positive category	Negative category	Positive category	Negative category
a	15	50	25	50	33	50	50	50	75	50
$\bar{a}$	42.5	25	37.5	25	33	25	25	25	12.5	25
Mean trials to solution	44.1		44.5		56.2		16.8		6.25	

Note. The percentages are exactly the same for both relevant dimensions A and X when the conceptual rule is biconditional; a = relevant value, $\bar{a}$ = irrelevant value.

The design of the experiment was a $5 \times 2 \times 2$ factorial, with five frequency relationships (see Table 3), two problems, defined by different pairs of relevant attributes (1–yellow and small–square), and an equal number of male and female subjects.

Procedure. In an effort to simplify the problem, subjects were given more extensive instructions and preliminary practice in the way in which stimulus patterns are categorized according to a truth table. The trial-by-trial routine was, however, essentially the same as that used in Experiment 1. On each trial, subjects were shown a pair of slides and were instructed to pick the positive one. After their response, the negative slide disappeared and the positive one remained in view for 5 sec. Trials continued until the subject made 12 consecutive correct responses. In view of the difficulty of the biconditional rule, the subject was guided through to solution of one practice problem by the experimenter before beginning the experimental task.

Results

The mean number of trials to criterion for the five main conditions of this experiment are shown in Table 3. In an analysis of variance, only the difference among these five conditions was statistically significant, $F(4, 40) = 10.66$, $MS_e = 329.06$. The outcome was the same for errors to solution.

The results are relatively clear-cut. The within-category differential is the most important frequency cue available to the subject in this experiment. When there is no frequency differential within the positive category, performance is worst. A facilitative effect exists when $\bar{a}$ (or $\bar{x}$) is more frequent within the positive category than a (or x), but the effect is small. Considerably more important is the case in which a (or x) exceeds $\bar{a}$ (or $\bar{x}$); furthermore, the larger the size of this differential, the greater the facilitating effect. If frequency is the only effective cue, then the subject must rely on a differential within the negative category in order to solve in Condition 3. All subjects solved this problem, but it was clearly the most difficult case.

The results also speak to the ineffectiveness of between-category frequency cues. If between-category differences were an important bases of discrimination, we would expect Condition 4 to be considerably more difficult, for no such differentials exist in this case. All values taken from the two relevant dimensions occur with equivalent frequency in both the positive and the negative category. The results are clearly contrary to this expectation.

In general, then, these data indicate that the within-category difference between a (or x) and $\bar{a}$ (or $\bar{x}$) is the frequency cue that subjects rely on primarily to identify the relevant values of a concept. The effectiveness of this differential is essentially limited to the positive category. Where no such differential exists in the positive category, performance is poor. While there is a small effect due to a differential favoring $\bar{a}$ and $\bar{x}$, the within-category difference is clearly most potent under the complementary case.

EXPERIMENT 3

We were concerned that the effectiveness of within-category differences in frequency, where values $\bar{a}$ and $\bar{x}$ exceed a and x, respectively, may have been minimized in Experiment 2 due to the particular frequency values selected. Differentials in favor of a and x in Conditions 4 and 5 of that study might have been more effective because they were numerically greater than differentials in favor of $\bar{a}$ and $\bar{x}$ in Conditions 1 and 2. Therefore, we undertook an extension of Experiment 2 designed to use more extreme frequency relationships in the positive category favoring values $\bar{a}$ and $\bar{x}$. Indeed, as shown in Table 4, we developed one extreme condition in which the relevant attributes never appear on the positive stimuli.

Method

Subjects. The subjects were 36 male and female undergraduate students in introductory psychology courses who participated for course credit. None had previously served as a subject in a concept learning experiment.

Design. The design consisted of three groups, 12 subjects per group, differing in value frequencies. An equal number of males and females were assigned to the three groups. Groups were also subdivided such that an equal number of subjects solved each of two experimental problems.

The procedure and apparatus were the same as those used in Experiment 2.

TABLE 4

Percentages of Occurrences of Value from Relevant Dimensions in Concept Categories in Experiment 3

Value	Condition 1		Condition 2		Condition 3	
	Positive category	Negative category	Positive category	Negative category	Positive category	Negative category
a	20	50	10	50	0	50
$\bar{a}$	40	25	45	25	50	25
Mean trials to solution	51.0		44.3		38.4	

Note. a = relevant value, $\bar{a}$ = irrelevant value.

Results

The frequency differential used over the three conditions of this experiment are presented in Table 4, along with mean trials to criterion. In an analysis of variance, only the effect of frequency conditions produced a significant effect, $F(2, 30) = 5.42$, $MS_e = 112.9$. Decreasing mean trials across conditions does suggest that subjects can use within-category differentials even when the frequency of a complement exceeds the primary value on a dimension in the positive category. However, the effect is small relative to the effects observed when the primary is more frequent, as in Experiment 2. Errors to solution exhibited the same outcome.

EXPERIMENT 4

In the first three experiments, we manipulated the frequency with which various values occur in both positive and negative instances of a concept. We have shown that one type of frequency differential—the contrast between a relevant or primary value and the irrelevant or secondary values from a relevant dimension—has a substantial effect on performance. Thus, the subject somehow manages to retain within-category frequency information for values along stimulus dimensions. Extending the general ideas of frequency theory (Ekstrand et al., 1966), we can argue that subjects periodically scan this frequency information or the "counters" on which it is registered in an attribute identification problem in search of significant differentials. The subject's response on any trial is based on whatever dimensions exhibit recognizable differentials. According to a frequency analysis, the dimension(s) hypothesized to be relevant on a given trial is (are) the dimension(s) showing the largest frequency differential(s).

A variety of possibilities exist for the way in which frequency information accrues to stimulus dimensions. While it seems unlikely, the subject might attempt to keep track of the frequency of all stimulus values within both the negative and the positive categories. If this were the case, the subject would come to each new trial with two independent within-category records based on the events of preceding trials. He or she would try to remember the two stimuli presented on that trial such that, at the time of feedback, both positive and negative counters could be augmented appropriately. Alternatively, the subject might focus on the positive category, entering each trial with a record from previous trials of the frequency of attributes within the positive category. Remembering both stimuli, the subject makes changes in frequency counters based on only the positive stimulus at the time of feedback. Because in the preceding experiments, feedback reinstated only the positive instance on each trial, there is still a third possibility. The subject could ignore the two stimuli initially presented on any trial and make his or her frequency adjustments solely on the basis of the positive stimulus identified at the time of feedback. It is conceivable, of course, that counts are made on the basis of both the initially presented stimuli *and* on the stimulus presented at feedback.

The present experiment examined the question of when frequency counts are made. Recall, from Experiment 2, that decreasing the size of frequency differentials within the positive category of a biconditional problem increases the number of trials required to solve. One explanation of this finding is that subjects make frequency counts only on the feedback stimulus, which, in that study, was always a member of the positive category. If the subject makes counts only on feedback stimuli, then, reinstating the negative stimulus as feedback should eliminate frequency effects when only frequency within the positive category is manipulated. If, on the other hand, frequency variations within the positive category affect performance significantly under these conditions, it indicates that frequency counts are made on the initially presented pair of stimuli as well as, or instead of, the feedback stimulus.

Method

Procedure. In all respects, the task for subjects in the present experiment was exactly the same as that required in earlier studies, with one exception. For half of the subjects, feedback on each trial was provided by removing the negative stimulus and allowing the positive stimulus to persist after the subject had made his or her response. For the remaining subjects, the positive stimulus disappeared upon the subject's response and the negative stimulus persisted. Subjects were told about the form in which feedback would be delivered during instructions and were given several practice trials to be sure that they understood whether the feedback stimulus was positive or negative. All subjects solved one biconditional attribute identification problem.

Design. The design of the experiment was a 3 × 2 × 2 factorial, with three frequency differential conditions, two forms of feedback (as described above), and two different problems, each with a different pair of relevant attributes. The frequencies employed are shown in Table 5. Six subjects were randomly assigned to each cell of the design, with the total number of subjects being 72.

Results

Mean trials to criterion for the six main frequency differentials by feedback condition are shown in Table 5. An analysis of variance indicated that only the frequency condition, $F(2, 48) = 87.44$, $MS_e = 441.74$, was statistically significant. There does tend to be a compression of frequency effects when the negative stimulus is used as feedback. But that effect is neither numerically impressive nor statistically significant. The interaction of frequency condition by feedback was unreliable, $F(2, 48) = 1.11$, while the difference among frequency conditions given negative feedback was highly significant, $F(2, 48) = 54.33$. The effects of frequency are, incidentally, exactly like those found in two preceding experiments and thus serve to corroborate those data. Again we find that differentials favoring a over $\bar{a}$ are more valuable to the subject than differentials in the other direction. The results were identical with errors to solution as the performance measure.

EXPERIMENT 5

The results of Experiment 4 indicate that frequency adjustments are made, at least partly, on the basis of the initially presented stimuli of any trial. In retrospect, our use of the biconditional rule was unfortunate because it might have precluded observable effects attributable to the feedback stimulus. The reason is that frequency differentials are not changed by the type of feedback in a biconditional problem, even though frequency totals are affected. Because both a and $\bar{a}$ values appear in the positive category of a biconditional, repeating positive instances at feedback adds extra counts proportionately to both a and $\bar{a}$. Consequently, the difference between the values on the relevant dimension is not increased. Furthermore, and perhaps for the same basic reason, subjects appear to gain roughly equivalent amounts of information from positive and negative instances of a biconditional concept (Bourne & Guy, 1968). In that sense as well, then, the subject may be indifferent to which type of instance serves as feedback in a biconditional problem.

Not all rules operate on attribute frequencies in the same way. In a conjunctive problem, for example, $\bar{a}$ values never occur in the positive category, and therefore extra counts resulting from positive feedback double the frequency difference between a and $\bar{a}$. After 10 trials of a conjunctive problem, for example, the a value would have a frequency total of 20 when positive stimuli are used as feedback, but only 10 when negative stimuli are used. Because they never occur in the positive category, $\bar{a}$ would have a frequency count of 0 under either feedback condition. If our analysis is correct, and subjects do make frequency adjustments on the basis of the feedback stimulus, positive feedback should facilitate the learning of a conjunctive concept relative to negative feedback.

The present experiment examines the importance of the type of feedback stimulus in a conjunctive concept. Using the conjunctive concept will allow us to gather evidence on another issue, which has been uncertain in previous experiments, namely, Do subjects maintain counters for both the positive and the negative category? The fact that subjects tend to focus on positive instances even in problems where they are not more informative than negative instances (Bourne et al., 1969) leads to an expectation that counters are established and/or scanned only in the positive category. In a conjunctive concept, unlike a biconditional concept, we can manipulate frequency differentials within the negative category. Effective frequency variations in the negative category would suggest that subjects do maintain counters for negative as well as positive stimuli.

Method

Procedure. The procedure of the experiment was essentially the same as that used in Experiment 4. The subjects solved one problem, based on a conjunctive combination of two values from two relevant dimensions.

The design of the experiment was a 3 × 2 × 2 factorial. Seventy-two subjects were distributed randomly but equally among the cells. There were three different frequency relationships between relevant and irrelevant values within the negative category, as shown in Table 6. Different groups of subjects were given the positive stimulus from each trial or the negative stimulus from each trial as feedback immediately after a response had been made. Finally, subjects were divided equally between two problems, each characterized by a different pair of relevant attributes.

TABLE 5

CONDITIONS AND RESULTS OF EXPERIMENT 4

Percentages of occurrences of values on relevant dimensions

	Condition					
	1		2		3	
Value	Positive category	Negative category	Positive category	Negative category	Positive category	Negative category
a	12.5	50	50	50	87.5	50
$\bar{a}$	43.8	25	25	25	6.3	25

Mean trials to solution

	Condition		
Feedback	1	2	3
Positive feedback	49.6	25.8	12.4
Negative feedback	47.0	25.6	16.4
Overall	48.3	25.7	14.4

Note. a = relevant value, $\bar{a}$ = irrelevant value.

Results

Mean trials to solution are shown in Table 6. As is obvious from the frequencies, the subject has a very effective and powerful frequency cue in the positive category. Both relevant attributes occur on every trial in the positive stimulus. None of the irrelevant values do. Therefore, on the basis of frequency argument, we would expect solution to be rapid regardless of other variations in the experiment. Indeed, it was, relative to performance on biconditional problems in earlier experiments. There is some, but totally insignificant, variation in the means of Table 6. Frequency effects are consistent with the differentials that exist but are unreliable, $F(2,\ 48) = 1.83$, $p = .17$, $MS_e = 23.26$. The subject has greatest difficulty when the relevant value occurs equally with irrelevant values in the negative category. When a discrepancy between relevant and irrelevant values exists, performance is slightly but insignificantly better. There is a mean difference between positive and negative feedback in the expected direction. The subject in this case, as in other data, appears to be able to use positive instances more effectively than negative toward solving a conjunctive problem. However, once again,

TABLE 6

CONDITIONS AND RESULTS OF EXPERIMENTS 5 AND 6

Percentages of occurrences of values on relevant dimensions

Value	Condition 1 Positive category	Condition 1 Negative category	Condition 2 Positive category	Condition 2 Negative category	Condition 3 Positive category	Condition 3 Negative category
a	100	15	100	33	100	45
$\bar{a}$	0	42.5	0	33	0	27.5

Mean trials to solution in Experiment 5

Feedback	Condition 1	Condition 2	Condition 3
Positive feedback	5.7	6.0	3.8
Negative feedback	7.0	9.2	6.4
Overall	6.4	7.6	5.1

Mean trials to solution in Experiment 6

Feedback	Condition 1	Condition 2	Condition 3
Positive feedback	5.7	9.8	3.7
Negative feedback	8.1	17.3	11.7
Overall	6.9	13.6	7.7

Note. a = relevant value, $\bar{a}$ = irrelevant value.

the statistical significance of this result is only marginal, $F(1, 48) = 2.13$, $p = .15$. We expect that this difference is real and could be demonstrated with a more powerful experiment and/or by using problems of greater difficulty. No other sources of variance in the analysis approached statistical reliability. The analysis of errors showed a similar performance pattern.

EXPERIMENT 6

The results of Experiment 5 provide little evidence that subjects maintain counters in the negative category or that they make extra counts on the stimulus provided as feedback. Nevertheless, trends in the data are consistent with both claims. Part of the problem might stem from the fact that conjunctive problems are almost trivially easy in the simultaneous discrimination paradigm. Frequency effects in the negative category and facilitation produced by extra counts at feedback might be obscured by a ceiling on performance. One possible way to increase the difficulty of the problem and at the same time emphasize the importance of the feedback stimulus is to interpolate a delay interval between the subject's response and presentation of feedback.

Method

The experimental design and the task were the same as in Experiment 5 with two exceptions. A 5-sec interval was interpolated between the subject's response and presentation of feedback. In addition, within the color dimension, blue was substituted for green, and within the shape dimension, hexagon was substituted for circle. The solutions remained 1–yellow and small–square. As in Experiment 5 frequency relationships within the negative category and the type of feedback were manipulated in a between-subjects design.

Results

Both errors and trials to solution displayed the same pattern of results. Means on the latter measure are shown in Table 6. Overall, solutions were achieved rapidly, though not as rapidly as in Experiment 5. There was a significant effect of feedback in the predicted direction, $F(1, 60) = 4.62$, $MS_e = 140.22$, 6.4 mean trials to solution for positive feedback and 12.4 for negative. This difference supports the notion that extra counts are made on the stimulus presented at feedback which produces larger frequency differentials for subjects given positive feedback.

The relationship among the means was identical to that obtained in Experiment 5 with one possibly significant exception. Generally, we have observed that a frequency differential favoring a over $\bar{a}$ is more effective than $\bar{a}$ over a. In the present experiment, when the negative instance of a trial was used as feedback, subjects performed better when $\bar{a} > a$ (Condition 1) than $a > \bar{a}$ (Condition 3). While the data are not extensive enough to support a confident conclusion, we might speculate that the more effective $a > \bar{a}$ cue is limited to the category of positive instances and that, when important differentials appear in the negative category, $\bar{a} > a$ is more informative to the subject.

Although these results are in line with our predictions, an analysis of variance revealed only a marginally significant frequency effect, $F(2, 60) = 2.27$. Once again it seems that subjects maintain counters within the negative category, but that conjunctive problems are too insensitive to reveal these effects clearly. A 5-sec delay of feedback task should have induced some forgetting of both initial stimuli, forcing the subject to rely on the physically present feedback stimulus for his or her frequency counts. This leads to the expectation of a Frequency × Feedback interaction; reliance on positive instances should produce no frequency effect, while negative feedback should enhance effects. The absence of such an interaction, $F < 1.0$, suggests that the degree of forgetting of initial stimuli was no greater in this experiment than it was in the previous one. Consequently the lack of a significant frequency effect is insufficient grounds for abandoning the idea of counters being maintained in the negative category, a notion supported at least by trends in the data of two separate experiments.

EXPERIMENT 7

In a final attempt to provide clear evidence of the importance of frequency counts in the negative category, we returned to a study–test procedure used by Bourne and Guy (1968). In that study, when subjects were forced to base their frequency counts exclusively on negative instances, the universally easy rule, conjunction, was the most difficult problem to solve. Negative instances provided no frequency differentials among stimulus attributes, which made the conjunctive problem, according to our argument, virtually insoluble.

It is possible to manipulate frequency differentials within negative instances in the study–test paradigm. In the present experiment, we replicated the small differential arrangement used by Bourne and Guy (1968) for purposes of comparison with two other conditions in which $a > \bar{a}$ and $a < \bar{a}$ in frequency. The latter groups should provide information about the relative effectiveness of differentials favoring a values as opposed to $\bar{a}$ values among negative instances. Even though both groups should theoretically solve faster than the group given only a small differential, it may be that the *less* often a values occur in the *negative* category the more effective the differential is. This result would be, of course, precisely the opposite of frequency effects observed heretofore in the positive category.

Method

Subjects. A total of 42 college students participated for cash payment. Several group sessions were conducted with all subjects attending a particular session ($n = 2$ to 6) assigned to the same experimental condition. One such subgroup ($n = 4$) was unique. Even before scoring the protocols of these subjects, the experimenter suspected that they had overheard the correct solution given to the preceding group at the end of its session. Suspecting that they knew the solution at the outset, we replicated their experimental condition with a second subgroup ($n = 6$). Performance in the replication was entirely consistent with data obtained from other subgroups serving under the same condition. While the study was statistically analyzed with both the original and the replicated subjects, we elect to base our primary conclusions on the replication. Subjects were assigned in equal number to six experimental groups, defined by the factorial combination of three frequency conditions, as shown in Table 7, and two problems (1-yellow and small-square).

Procedure. A series of alternating study and test trial sequences was designed for each condition. A study sequence consisted of eight successively presented negative instances of a conjunctive concept, with the composition of the sequence varying according to frequency manipulations. For example, in Condition 2 there were two TF, two FT, and four FF instances in each sequence. For Condition 1 the composition was one TF, one FT, and six FF instances. For Condition 3 there were either three or four TF or FT instances and always two FF instances presented. The test sequences consisted of a member from each of the four truth-table classes; that is, there were three negative stimuli and one positive within each group of four. The composition of study and test sequences was, of course, unknown to the subject although each study instance was identified as negative.

As in previous experiments the conjunctive nature of the solution and the dimensions along which the stimuli varied were explained before beginning the task. Then a study sequence of eight negative instances was shown at the rate of one every 5 sec. Next the first four test stimuli were presented one at a time for 10 sec. Subjects responded to each by writing + or − on an answer sheet. This study–test series was repeated 15 times; hence overall, subjects studied 120 negative stimuli and responded on 60 test trials.

Results

Because the number of test trials was fixed for all subjects, the number of errors was the main performance index. The data

TABLE 7

PERCENTAGES OF OCCURRENCES OF VALUES ON RELEVANT DIMENSIONS IN CONCEPT CATEGORIES IN EXPERIMENT 7

Value	Condition 1		Condition 2		Condition 3	
	Positive category	Negative category	Positive category	Negative category	Positive category	Negative category
a	100	15	100	25	100	45
$\bar{a}$	0	42.5	0	37.5	0	27.5
Mean errors	22.1		28.4		24.6	

Note. a = relevant value, $\bar{a}$ = irrelevant value.

are presented in Table 7. The only reliable effect in an analysis of variance was frequency, $F(2, 30) = 4.80$, $MS_e = 25.46$; problems and all interaction sources of variance were insignificant. Planned orthogonal comparisons showed that Condition 2 made significantly more errors than the average of the two frequency differential conditions (1 and 3), $F(1, 30) = 8.12$. The latter two conditions do not differ reliably, $F(1, 30) = 1.47$. The first result indicates that subjects apparently can maintain and scan counters within the negative category, at least when forced to do so. The large performance difference in favor of subjects provided with frequency cues confirms the trends found in both Experiments 5 and 6. Also, although the difference was unreliable, subjects given differentials favoring $\bar{a}$ values made fewer errors than those given $a > \bar{a}$. Unlike the case in which differentials are varied within the positive category, it seems that $\bar{a} > a$ differentials are more effective in the negative category, when negative feedback or negative study instances are used.

DISCUSSION

The results of these experiments support the following conclusions. Differences in the frequency of occurrence represent a potent source of information which subjects use to identify the relevant values of an unknown concept. While there are a variety of frequency differentials available, the most effective cue is provided when the relevant concept value occurs within a particular category with a substantially different frequency than other values on the same stimulus dimension.

The frequency theory of verbal discrimination learning (Ekstrand et al., 1966) can be extended to attribute identification in such a way as to give a natural account of these results. According to frequency theory, the primary basis for learning discriminations is the accumulation of differential frequency (of occurrence) counts for the correct (i.e., relevant) versus incorrect (i.e., irrelevant) stimulus or stimulus values. For the present experiments, the theory would suggest that the subject sets up within-category frequency counters for each value on each stimulus dimension. The subject scans these counters periodically in search of significant differentials. Once a differential is detected, the subject attends to and responds primarily on the basis of the dimension within which that differential exists. Results obtained here suggest that subjects are capable of using counters within both the positive and negative categories of a concept, but apparently focus whenever possible on positive counters. This fact comes as no great surprise in view of earlier research in concept formation which documents a strong tendency toward positive focusing in most subjects.

Analogues to the Ekstrand et al.'s (1966) Rule 1 and Rule 2 are also evident in the data. That is, it would appear as if the subject can use differentials which favor either the defining value, a (Rule 1), or the nondefining values, $\bar{a}$ (Rule 2), from a relevant dimension. We have shown that Rule 1 is clearly easier to follow when frequency variations are made within the positive category. However, $\bar{a} > a$ differentials seem to be the more useful when the differentials appear in the negative category. This dependency on category can be understood in the light of data reported by Toppino and Johnson (1974). These authors show that when subjects are given only positive items in a study–test paradigm, they adopt a strategy of looking for the *occurrence* of a relevant attribute, while subjects given only negative instances look for the *absence* of a relevant attribute. Thus under the former conditions, a differential favoring $a > \bar{a}$ would focus subjects' attention on the relevant attributes, while an $\bar{a} > a$ differential would be more useful when subjects are provided with only negative information to work with as in Experiment 7.

Our results suggest that frequency counts accumulate primarily during stimulus presentation, but also accumulate during feedback. The data indicate that frequency counts are made separately for the two concept categories. The subject attempts to retain the attributes of each stimulus presented on a given trial until feedback. At feedback, adjustments are made in both counters. Because subjects focus on positive instances, counts within the positive category tend to be more accurate than those within the negative category. Subjects can effectively use negative differentials when required, however. Furthermore, because subjects give extra weight (frequency counts) to the stimulus used as feedback, the tendency to positive focus is moderated when feedback is provided by negative instances.

From these experiments, we conclude that frequency theory and frequency effects account for a wide range of variables in concept formation. This may not be surprising, in view of the fact that concepts are, in the last analysis, based on discriminations among objects, events, or processes in one's environment, and frequency theory has already been shown to provide a broad-gauged interpretation of discrimination learning phenomena (Ekstrand et al., 1966). On the other hand, the wide array of effects more or less unique to concept formation which frequency theory gives a reasonable account of is, we think, truly impressive. Consider the variables examined in the present series of experiments. Foremost among them is the form of the conceptual rule. It has been known for some time that bidimensional rules are not equivalently difficult in attribute identification problems, despite the fact that the rule is named in advance for the subject. A variety of interpretations have been offered, as summarized elsewhere (Bourne, 1970). None of these interpretations has been able to make sense consistently out of the available data. We were alerted to a possible frequency interpretation by an unusual outcome in one earlier experiment. It is reasonably clear, from the first of the studies reported in this article, that, if nothing else, the difficulty of required discriminations imposed by the various conceptual rules correlates reliably, indeed almost perfectly, with the frequency differentials that accumulate between relevant and irrelevant values of the stimulus among instances of the positive category. Subsequent studies give a clearer picture. Within the context of a single rule, one can manipulate difficulty widely from practically trivial to practically impossible just by arbitrarily changing the aforementioned frequency differentials. We conclude that frequency theory is the most adequate interpretation of rule differences yet suggested.

We have found elsewhere (Bourne & Guy, 1968) that the utility of positive and negative instances is a function of the conceptual rule. Positive instances are a much more valuable source of information when the concept is conjunctive, while negative instances are more informative and more effectively used when the concept is conditional. An examination of frequency differentials within the positive and the negative category reveals a correlate of this phenomenon. For conjunctive problems, a large frequency differential exists within the positive category, relative to differentials within the negative. Just the opposite is the case in conditionals. For disjunctives and biconditionals, frequency differentials are relatively equivalent in both categories. As a frequency analysis predicts, subjects are relatively indifferent to the positive–negative distinction in these latter two cases.

There are several factors affecting concept formation, which have been documented elsewhere, for which frequency theory also gives an adequate interpretation. Haygood, Harbert, and Omlar (1970) demonstrated that, under certain conditions, an increase in the number of values per dimension of the stimulus leads to *improvement* in attribute identification performance. This outcome is somewhat counterintuitive because intradimensional variability contributes to the complexity of the stimulus population, and factors of that sort have been shown elsewhere (Bourne & Restle, 1959) to impede attribute identification. The fact of the matter is, however, by increasing intradimensional variability, Haygood et al. simultaneously increased the magnitude of the frequency differential between the relevant value of a given concept and all irrelevant values. Chumbley, Lau, Rog, and Haile (1971) and Schultz and Dodd (1972) have shown that by increasing the frequency of a selected value on *irrelevant dimensions* of the stimulus, so as to counterbalance frequency differentials produced by intradimensional variability, all or nearly all of the intradimensional variability effect is removed. Bourne and Hanson (Note 1) have shown that artificially increasing the frequency of a selected irrelevant value from the *relevant* dimension, making the difference between it and the relevant value smaller, also minimizes the intradimensional variability effect. Thus, it seems clear at this point that frequency differentials were probably the base factor relied upon by subjects in the Haygood et al. experiment and, correspondingly, that frequency theory can give a reasonable account of the effects of intradimensional variability.

In contrast to the aforementioned effect, increasing the frequency with which a particular value from an irrelevant dimension occurs in the positive category—a manipulation which has been called percentage of reinforcement for an irrelevant dimension (Bourne & Haygood, 1960)—facilitates the identification of the true relevant value. The effect can be understood as follows. The relevant value of the concept occurs with 100% frequency in the positive category. The more frequently an irrelevant value occurs in the positive category, the more the two values, one relevant and one irrelevant, come to function as a larger, more salient single one. It is well known from other experiments (Bourne & Haygood, 1959) that when two values from different dimensions occur 100% of the time in the positive category, that is, when there are two redundant cues to solution, performance is markedly facilitated. Thus, as the frequency of an irrelevant cue within the positive category increases, the situation becomes more like the redundant relevant-cue problem. We would say that, as the frequency of the irrelevant value in the positive category becomes large, the subject is provided with two redundant, rather than one, frequency discriminations. These differentials draw the subject's attention to the two critical values. The more reinforcement for an irrelevant value, the quicker the subject's attention is drawn to the pair. From that point on, what was once a problem which required discrimination among some large number of stimulus values becomes a matter of separating the correct from the single incorrect value, a much simpler problem.

It is one thing to give a post hoc account of known results. It's quite another to use the same theory to forecast various phenomena and relationships. We think that a frequency theory of attribute identification has that potential, and we conclude by offering several possibilities for future research. Having postulated that subjects in some way keep track of attribute frequency, we would expect that, at the time of concept attainment, and perhaps even before, they will be able to estimate the relative frequency with which values on the relevant and irrelevant dimensions occur within the positive category. We expect some degree of absolute accuracy in these frequency judgments. Furthermore subjects who are accurate in their frequency judgments, either in the concept identification task or in a different context, should be better concept learners than those who are inaccurate in a frequency judgment task. Although frequency judgment data would directly bear on the issue of whether frequency information is maintained in an accessible fashion, these data would not reveal how its encoding is accomplished. Ekstrand et al. (1966) use the notion of counters which increment each time the appropriate stimulus or stimulus feature is encountered. Such counters also provide a convenient metaphor, useful in expressing the implications of our frequency theory. At this stage, however, we prefer a more flexible position, for it is not at all clear how frequency information is actually encoded and stored. Consequently, our metaphorical use of the counter idea should not be construed as a definite theoretical commitment. To the contrary, the basic ingredient of our theory, the frequency differential, would function appropriately assuming any one of a variety of representations of frequency information.

By varying the frequency with which one value from each irrelevant dimension occurs within the positive category, relative to all other values on the same dimension, we can predict the order in which a subject will eliminate each dimension as irrelevant. First, the more evenly frequency of occurrence in the positive category is spread over the values of a dimension, the easier that dimension will be to eliminate; the greater the frequency differential between one value and all others on the same dimension within the positive category, the more difficult it will be to eliminate. Second, and related to the first hypothesis, we ought to be able to select an optimal sequence and varying degrees of nonoptimal stimulus sequences. An optimal sequence is one which spreads frequencies evenly over irrelevant dimensions while maximizing the differential within the relevant dimension. Nonoptimal sequences are those which produce a frequency differential within one or more irrelevant dimensions.

One final point might be made about the frequency theory of attribute identification. We believe there is some possibility that this theory can give a more adequate account of the formation of naturalistic concepts than any other yet proposed. As noted by Rosch (1973) and others, there are important intrinsic differences between natural concepts and those ordinarily studied in the laboratory. For one thing, the dimensions which characterize the natural environment are not neat and tidy orthogonal dimensions, each with an equivalent number of values. Rather, they tend to form correlated clusters. Many of them are continuous rather than discrete. Instances of a natural concept may not share any particular attribute. Even in a simple base-level concept like *table,* there may be no single physical or perceptible value that all instances have. Ordinarily, a person can know and use a naturalistic concept without awareness of all, or perhaps any, of its critical features. Finally, naturalistic concepts often seem best represented by some prototypical or "best" instance. Other instances of the same concept have varying degrees of membership. Some are marginal, some are almost as good as the prototype. The formation and use of natural concepts has become the object of recent investigation (Rosch, 1973), but not within the context of the traditional laboratory task. The available evidence suggests that people do indeed typically form concepts around a prototypical value and can offer judgments regarding the degree of membership with respect to varying instances.

We believe that a reasonable interpretation of these facts can be given as follows. A concept is defined, or parameterized, by some rule form-relationship among a set of attributes, a_1, a_2, a_3,... (Bourne, 1970). Each attribute has some frequency of occurrence in the positive category of a concept. No attribute necessarily occurs with probability 1.0; however, the more probable an attribute, the better it is as a defining characteristic of the concept. In this scheme, the prototype would be defined as that stimulus which contains all defining characteristics, no matter how low their probability. Where two defining characteristics cannot coexist, the prototype will contain the more highly probable. The degree of membership for any particular instance is controlled by which and how many defining characteristics that instance has. In general, the more defining characteristics there are, the better the instance is as a representative of the category. Further, given two instances with the same number of defining characteristics, the one whose characteristics have higher frequency on the average will be judged a better member of the category. We plan an experiment in which a new, quasi-natural concept will be taught to subjects. The concept will have a great many defining characteristics whose frequency of occurrence in the positive category will vary. After concept learning, we plan to elicit judgments from the subject about the prototype or best example of the concept, about the relative merits of any pair of instances as to category membership, and about the rank ordering of randomly selected instances as to their representativeness. We expect to be able to reproduce the phenomena said to be characteristic of natural concepts in this laboratory situation and thereby to give additional evidence in support of a frequency analysis and theory of concept learning.

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An Interval Scale for Studying and Quantifying Social Relations in Pairs of Rhesus Monkeys

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SUMMARY

An interval scale of 17 behavior categories was constructed from data on 120 pairs of rhesus monkeys while they were establishing a social relationship. Data were obtained by a procedure analogous to that used in human psychophysical scaling. The Method of Successive Categories was used to obtain interval values of these categories from 4,662 spontaneous social interactions occurring as two step chains—a single dominant stimulus category followed by a single submissive response category, preceded and followed by at least 15 sec of no social interaction. A second group of social interactions occurred as long-step chains—three or more behavior categories preceded and followed by at least 15 sec of no interaction. Category intervals derived from two-step chains were used to determine the appropriate constant which would make the sums of all long-step chains approximate zero. This constant was then used to derive the final scale values.

This scale was applied to the long-step chains to analyze the possible meaning of these complicated sequences. It was found that these sequences could be empirically placed in one of five classes based on whether one or both animals showed only dominant or submissive categories or a mixture of the two. Such a mixture was found to signify friendliness and was used by either monkey to attract the other.

Dominance hierarchies in rhesus monkeys have been well documented in field reports and should provide a good social system for detailed study of such constructs as agression, fear, and social closeness. However, it is difficult to compare different social relationships without a quantitative scale for measuring such social interaction (Maxim, 1972). Most studies of social relationships in monkeys have concentrated on developing nominal behavior scales such as catalogs (Hinde & Rowell, 1962) which simply classify behavior categories and indicate measures of frequency. The next higher order of scaling is a rank-ordered or an ordinal scale of behavior categories (reported for rhesus monkeys by Altmann, 1962) and a derivation of this scale is a useful beginning measure (Maxim, 1972).

In the literature, primate behavior categories are often ranked in terms of a characteristic emotional state such as increasing aggression or fear. This principle permits the use of additional statistics such as medians and rank-order coefficients of correlation. A median divides combined frequencies into equal quantities which can be compared, but finer comparisons in the rank ordering are unjustified, which severely limits analysis of social behavior. Studies too often rely on a single, frequently occurring measure to symbolize a complex relationship, or report single frequency measures across a number of behavior categories in a single time block. Both procedures miss the detailed sequencing of multiple behavior categories within the time block which are important for measuring behavioral change in relationships.

This article presents an interval scale which allows one to use the whole repertoire of rhesus social behavior in measuring dominant–submissive relationships. Interval scales are equal-unit scales in that numerically equal distances on the scale stand for empirically equal distances in some aspect of the categories concerned. With such a scale one can add intervals expressed by the categories on it and logically arrive at an overall score of the underlying property. Furthermore, one can compile a "running score" of behavioral interaction which will reflect social changes. The meaningful addition of such amounts requires a scale with an appropriate zero point; but scales of this type, such as scales of length or numerousness, are rare. Most scales of physical measure are interval scales which have logically but arbitrarily defined zero points, for example, calendar time. In the scale reported here the zero point is similarly derived by a logical method.

Maurus and Pruscha (1972) attempted to quantify social behavior in squirrel monkeys responding to telestimulation-induced behavior categories of a test monkey, and concluded that social interaction following a given category was too random to allow scaling. They may have had difficulties because their response data were gathered from multiple ongoing interactions of group members. As Carpenter (1964) noted, "the individual within a group does not have free behavioral exchange with every other member because of established channels of relations and restrictions and his own status." This makes it difficult for the observer to evaluate the real communicative effect of a given behavior category on a group of monkeys. Thus, in this study observations were made on the social interaction that developed in pairs of rhesus monkeys that were initially strangers to one another.

In preliminary observations it was clear that each animal produced certain behaviors

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These data were collected by personnel of the Aerospace Medical Research Laboratory, Aerospace Medical Division, Air Force Systems Command, Wright-Patterson Air Force Base, Ohio. The study was conducted according to the "Guide for Laboratory Animals Facilities and Care," 1965, prepared by the Committee on the Guide for Laboratory Animal Resources, National Academy of Sciences, National Research Council. Reprints of that article are identified by the Aerospace Medical Research Laboratory as AMRL-TR-73-98.

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Requests for reprints should be sent to Peter E. Maxim, Department of Psychiatry and Behavioral Sciences, University of Washington School of Medicine, Seattle, Washington 98195.

to express dominance and others to express submission, while mixtures of different behaviors sequentially produced seemed to signify friendliness. To all of these categories the responding animal produced a response which frequently seemed specific. Such specific single responses seemed readily usable in scaling the behavior categories. On the other hand, much of the subtlety of the relationship appeared to occur in longer sequences of interaction. In this article I first describe the use of long and short sequences of interaction in deriving a scale of behavior categories and then describe how the scale was used to examine the more complex messages occurring in the longer interaction sequences.

In the short interaction sequences each animal most clearly seemed to "judge" behavior categories produced by the other monkey and reply to them with a complementary response. For this reason it seemed possible to adopt a scaling strategy analogous to that used for human psychophysical scaling which utilized these short sequences.

Theoretic bases for unidimensional psychophysical scaling models have been summarized by Torgerson (1958). In psychophysical scaling, the human subject judges where a particular stimulus from a set of stimuli is to be rank-ordered on a given dimension. In our study sets of 3 monkeys of high, middle, and low rank were paired in turn with sets of 10 other monkeys. Because of their social rank it was presumed that the 3 "index" monkeys would together present the full range of behavior categories to the 10 other monkeys who, by their responses, would "judge" the rank-order of these categories. A scale value for a given category could then be established by relating the frequency distribution of responses for that category to the frequency response distributions of the other stimulus categories according to Thurstone's Law of Categorical Judgment. (The Law of Categorical Judgment is a set of equations which, using the frequency distributions of responses to a set of stimuli, establishes the parameters and mean scale values of those stimuli.)

This scaling model assumes that the underlying attribute to which the behavior categories are related is on a psychological continuum. Each time a category is presented it presumably elicits some sort of discriminal process whose value on this continuum is expressed by the complementary response category. The model also implicitly assumes that at the time of judgment the response category correlates with internal state. Due to a number of factors, including varying perception and internal mood states of the responding monkey, repeated presentation of the stimulus category is not always associated with a particular response category, but may be associated with ones higher or lower on the continuum. The model does postulate that the values associated with any given stimulus behavior category project a normal distribution on the continuum. Different stimulus categories may have different scale values and standard deviations to their response category distributions.

A number of methods that represent special cases of Thurstone's Law of Categorical Judgment have been developed to handle data from different experimental situations. In this experiment, behavioral response "judgments" were analyzed according to the Method of Successive Categories. This method used judgments which fall into a limited number of categories along the continuum. No assumption is made concerning the psychological equality of behavior category intervals but it is assumed that one can rank order the categories and that their boundary definitions are stable except for sampling errors.

It should be noted that this is a method for scaling behavior categories, not for assessing the characteristics of the monkey population. However, since the resultant scale was supposed to reflect "normal" emotional affect inherent in the behavior categories, we attempted to obtain as "normal" a population of rhesus monkeys as possible. Sackett (1965), among others, has presented evidence that the coupling of affect to a given behavior category is largely a learned phenomenon. Therefore, the "judges" in this study were adult rhesus monkeys who had had early social experience in the wild and so presumably could give accurate social responses. Some studies have also reported separate female–female hierarchies within the male-dominated group (Altmann, 1962; Carpenter, 1964; Hinde & Rowell, 1962). Therefore, to cover the full range of possible social interaction all three pair types were tested: male–male, male–female, and female–female.

In compiling a catalog of behavior categories I tried to keep the categories as molecular as possible so that they could be treated later as units of recombination in studying longer category sequences. This produced a more restricted definition of some categories (e.g., threat, lipsmack) than that contained in previous reports (Hinde & Rowell, 1962). A preliminary catalog, comprised of combined lists (Altmann, 1962; Hinde & Rowell, 1962; Sade, 1965), was used in several pilot sessions, and categories that were not observed were deleted. In addition, since communication was considered as a dynamic process, the behavior repertoire was limited to those categories which, when attempted, generally produced an immediate observable change in the behavior of the other animal.

METHOD

Subjects were 26 adult rhesus monkeys (*Macaca mulatta*): 13 males with a mean age of 7 years (range 6 to 12 years) and a mean weight of 9.7 kg (range 7.2 to 13.6 kg), and 13 females with a mean age of 5.75 years (range 4 to 11 years) and mean weight of 5.5 kg (range 3.7 to 6.8 kg). Animal handlers picked three index animals of each sex from a colony population of 250: the most dominant, the least dominant, and a middle-ranking animal. Ten other monkey "judges" of each sex

TABLE 1

BEHAVIOR CATALOG

No.	Category	Definition
1.	Attack	Running attack, mouth open, ears flattened, whether or not contact with the other is made
2.	Hit	Attempting to slap or hit at the other
3.	Bite	Attempting to bite the other while holding him down
4.	Open-jawed threat	Sudden forward head thrust, mouth opening wide, ears forward, shoulder and neck hair erect, while staring at the other
5.	Frown-threat	Sudden staring at the other, mouth closed, eyebrows moving up and down, ears flicking back and forth; looks "scandalized"
6.	Slap ground	Slapping perch with one hand while sitting, staring at the other
7.	Branch shake	Attempting to shake perch or cage bars
8.	Head bob	Sudden up-and-down head movement while staring at the other
9.	Approach	Walking toward the other without looking at him, starting from a distance greater than 3 ft. (.91 m)
10.	Stare	Looking intently at the other monkey while sitting, standing, or walking; fixing eyes on other's face, making eye contact should the other look around
11.	Mount	Attempting to mount the back, side, or shoulders of another; pelvic mounting except as defined for sexual mounting
12.	Sexually mount	Attempts by a male to pelvic mount a female, occurring as one of a sequence of 4–18 mounts generally 10–45 sec apart, between which the pair sits together
13.	Pace	Walking stiff-legged with arched back and raised tail, often flicking out front teeth with each step, not looking at the other monkey but obviously crossing his field of vision
14.	Ignore	Not giving visible reaction to a behavior directed toward oneself
15.	Look away	Looking to the side away from the other animal
16.	Walk away	Walking or trotting away from the other animal
17.	Show neck	Looking up and away from another monkey, exposing shoulder and neck to it
18.	Present	Presenting hindquarters to another except as defined in sexual presenting
19.	Sexually present	Presenting hindquarters by a female to a male when they are less than 3 ft (.91 m) apart, while she looks back at him through her legs or over her shoulder
20.	Lipsmack	Smacking of lips alternating with licking tongue movements while the face is turned toward the other monkey
21.	Grimace	Retracting lip, exposing teeth in a grin while turning head to the side
22.	Flee	Rapid running and jumping away from another animal
23.	Crouch	Sitting with shoulders hunched, arms bent, head down, and not moving in response to behavior by another
24.	Groom	Sliding, scraping, picking at the hair of one monkey by another
25.	Togetherness	Two animals sitting less than 3 ft. (.91 m) from each other, during which they may groom, huddle closely, or doze

TABLE 2

Distribution of Social Behavior Categories Across the 30-Min Observation Session, Obtained from Each of 10 Pairs of Male Monkeys

Successive 3-min intervals	Mean percentage
1	23
2	20
3	10
4	9
5	9
6	7
7	6
8	6
9	5
10	5

were randomly picked from the same population, excluding any that showed abnormal behavior such as motor tics. Though none of the population had been previously paired together, the choices of index monkeys proved to be accurate in terms of dominance status. The 6 index monkeys were paired with each of the 20 stimulus monkeys for two 30-min sessions occurring at least 2 weeks apart. Data were thus obtained from 240 sessions using 120 pairs: 30 male–male, 30 female-female, and 60 male–female pairs. All monkeys were at least 2 years old when caught in the wild; none had had prior social contact with others in the group. All males had had their canine teeth removed at least 1 year previously. Between test sessions males and females were housed in separate rooms, in individual cages from which they could hear but not see one another.

Pairs were introduced into a 6 × 6 × 6 ft. (1.83 × 1.83 × 1.83 m) observation cage for the 30-min test session at the same time each day. The cage was tile-enclosed except for the front side of steel bars, and was isolated from other rhesus monkeys. Its lighted interior was bare except for food and water supplies and a 2-ft. (.59-m) deep perch running the length of the back wall, 3 ft. (.91 m) off the floor. The monkeys could see, in addition to the seated observer, baboons caged 30 ft. (9.1 m) away, and could hear all the usual sounds of a large animal facility. In male–male pairings there were frequently several minutes of initial fighting until one male showed obvious and consistent submissive behavior. Scoring was begun at time zero, but data were not used for scaling until 1 min after initial fighting stopped. In pairing without initial fighting, data were used for scaling beginning 2 min after introduction. This rule was imposed because relationships prior to those time points were frequently confused.

Behavior categories were scored on a checksheet divided into 15-sec blocks; multiple categories occurring within a block were numbered serially. The categories in Table 1 were scored for either animal in reaction to the other.

These categories appeared to account for all observed behavior from a monkey pair with three exceptions: (a) vocalization—chewing, gnashing of teeth, calls, and grunts; (b) self-directed and solitary behavior—exploration of cage and contents, eating, drinking, yawning, and self grooming; (c) static behavior patterns—postural variations in sitting or walking. These exceptions had no clearly observable communicative value as judged by the lack of an immediate response by the pair mate.

The behavior categories were defined as "attempts" without regard for the response elicited, so that all categories that occurred would be scored. For example, if one animal attempted to mount another, it was scored as a *mount,* whether the other monkey fled on being touched or stood and presented.

Results

As previously noted, the observational data were first used to derive the interval scale of behaviors described below. The scale was then used to analyze the content of longer sequences of interaction.

When adult monkeys were paired the relationship typically began with intense interaction leading to establishment of a dominant–submissive relationship. This interaction consisted of two types of sequence: (a) a two-step chain of behavior categories in which each animal produced only one category, and the two categories were preceded and followed by at least 15 sec of no interaction; and (b) a long-step chain in which three or more categories were preceded and followed by at least 15 sec of no interaction. Tables 2 and 3 summarize the frequency distribution for 10 male–male pairings of these two types of chain sequences during a test session. There is a decrease in frequency of all sequences as the relationship progresses with an increase in the proportion of sequences scored as two-step chains (Table 3). Many such pairings were observed, suggesting that both animals were confused about their initial dominance–submission relationship and, as Altmann (1962) has noted, each monkey acted to maximize his gains and minimize his losses. Thus, as one monkey gradually manifested dominance, he became the initiator of most interactions and tended to produce increasingly aggressive categories unless the subordinate animal mollified him with submissive response categories. If he received suitably submissive

TABLE 3

RELATIVE PROPORTION OF TWO-STEP CHAINS AND LONG-STEP CHAINS IN EACH 3-MIN TIME BLOCK FOR 10 MALE PAIRS OF TABLE 2

Successive 3-min intervals	Mean percentage
1. Two-step chain	35
Long-step chain	65
2. Two-step chain	32
Long-step chain	68
3. Two-step chain	54
Long-step chain	46
4. Two-step chain	70
Long-step chain	30
5. Two-step chain	65
Long-step chain	35
6. Two-step chain	60
Long-step chain	40
7. Two-step chain	80
Long-step chain	20
8. Two-step chain	72
Long-step chain	20
9. Two-step chain	80
Long-step chain	20
10. Two-step chain	82
Long-step chain	18

Note: Of all categories, 65% occurred as two-step chains. Data were derived from 3,618 behaviors.

responses, the dominant animal then tended to produce less dominant or neutral categories that would attract the subordinate animal closer and eventually lead to mutual grooming. The subordinate monkey appeared to be as aggressive as possible in his responses short of eliciting aggression in return. These conflicting motivations resulted in initial long-step chains of category sequences as each animal sought a compromise in the relationship. With continued communication the subordinate monkey showed an increasingly restricted set of submissive responses to a behavior produced by the dominant; these responses appeared to be increasingly accurate opposites to a given dominance category, shutting off further categories from the dominant monkey. The result was a decrease in the number of long-step chains and an increase in two-step chains. These two-step chains could be defined as a dominant stimulus category followed by a submissive response category which was an accurate opposite judgment as perceived by both monkeys. Therefore, the interval scale was constructed using only two-step chains since both animals "agreed" that one response in these chains was an accurate "opposite" to one stimulus category. Since long-step chains were also presumed to end only when the sum total of behavior categories from one animal was equal to and opposite that of the other, this second set of chains could later be used to establish the zero point of the scale.

A total of 4,662 two-step chains were used to construct the scale with the Method of Successive Categories. For scaling, mounting and sexual mounting were considered a single category, as were presenting and sexual presenting. In addition, frown-threat and open-jawed threat appeared to be idiosyncratic to the given monkey rather than the social context, so both were combined as the single category *threaten*. Since accurate interval scaling required moderately large samples, categories that occurred fewer than 10 times in a grouping were not scaled. Accordingly, the categories *head bob, bite,* and *slap ground* were not scaled, and *branch shake* was scaled only for the two male–female groupings. *Crouch* and *show neck* could not be scaled for the male–male grouping. The final scale consisted of 17 behavior categories.

The scale was computed by the method of Guilford (1954). For each index animal and his 10 pair mates a frequency response matrix was constructed, plotting the stimulus categories of each index monkey against the response category of the pair mate "judge" as found in the two-step chains. To be scaled, the behavior categories first had to be in correct rank-order, accomplished by transforming the frequency matrix to a cumulative proportion matrix. The cumulative proportions were summed across matrix category rows, and the categories were rank ordered according to these sums. The cumulative proportions were then summed across all columns of categories and the categories rank ordered according to these sums. This rank-order was maintained for all subsequent matrix analyses. If the categories are indeed scalable, their frequency distributions should now cluster along a diagonal of the matrix (Torgerson, 1958). Figures 1–4 show that the responses do fall on the diagonal for all matrices when the categories are correctly rank ordered. Because of its social position, a single index animal frequently showed social interaction limited to only one part of the category range. Therefore, the data from all three index monkeys of a given type of pairing were combined, and this matrix provided the basic computational data. Even when combined in this manner, the data for the 3 index females paired against 10 males covered only the lower portion of the frequency response matrix, while the data for the index males

TOP RANK MALE

TEN MALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	IGNORE	LIPSMACK	LOOK AWAY	WALK AWAY	PRESENT	GRIMACE	FLEE
ATTACK														
HITS														
THREATEN														
MOUNT												23		
APPROACH												2		
STARE								9				1		
PACE														
IGNORE						5	2	14						
LIPSMACK						3	2							
LOOK AWAY				1	2	30	16							
WALK AWAY			1	4	25	17	5							
PRESENT			1	58	5	12	3							
GRIMACE			30	1	9	11	1							
FLEE			5	3	6									

307 TWO-STEP CHAINS

MIDDLE RANK MALE

TEN MALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	IGNORE	LIPSMACK	LOOK AWAY	WALK AWAY	PRESENT	GRIMACE	FLEE
ATTACK														2
HITS														3
THREATEN												1	22	1
MOUNT											2	7		
APPROACH										1	8	5	8	14
STARE								1		28	9	3	16	
PACE								3	4	11	7			
IGNORE						1		12						
LIPSMACK						1	4							
LOOK AWAY					1	9	2	5						
WALK AWAY				7	4	5								
PRESENT				69		15								
GRIMACE			9	1	3	9								
FLEE	4	1	4	7	3									

332 TWO-STEP CHAINS

LOW RANK MALE

TEN MALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	IGNORE	LIPSMACK	LOOK AWAY	WALK AWAY	PRESENT	GRIMACE	FLEE
ATTACK														23
HITS														10
THREATEN												3	21	21
MOUNT										1	10	26	1	8
APPROACH										14	23	8	24	13
STARE									4	52	34	26	33	
PACE								2	7	15	8			
IGNORE								11		11		1		
LIPSMACK									3					
LOOK AWAY						8	2	3		6				
WALK AWAY														
PRESENT														
GRIMACE														
FLEE														

432 TWO-STEP CHAINS

THREE MALES COMBINED

TEN MALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	IGNORE	LIPSMACK	LOOK AWAY	WALK AWAY	PRESENT	GRIMACE	FLEE
ATTACK														25
HITS														13
THREATEN												4	43	22
MOUNT										1	12	56	1	8
APPROACH										15	31	15	32	27
STARE								10	4	80	43	30	49	
PACE								5	11	26	15			
IGNORE						6	2	37		11		1		
LIPSMACK						4	6		3					
LOOK AWAY				1	3	47	20	8		6				
WALK AWAY			1	11	29	22	5							
PRESENT			1	127	5	27	3							
GRIMACE			39	2	12	20	1							
FLEE	4	1	9	10	9									

1071 TWO-STEP CHAINS

FIGURE 1. Frequency distribution of two-step chains occurring between 10 males and top-, middle-, and low-rank males. (Lower-right matrix shows combined frequency distribution.)

paired against 10 females covered only the upper portion of the matrix. These were combined into a single matrix. Scaling was thus carried out on three matrices: combined male–male, female–female, and male–female.

The categories were then scaled by finding their respective means and the distance between means. By assuming a normal distribution and knowing what proportion of response judgments to a given stimulus occurred *in* Category x and what proportion occurred *below* Category x, one could compute the mean of Category x. For each data point in the matrix a category mean was computed by the formula:

TOP RANK FEMALE

TEN FEMALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	IGNORE	LIPSMACK	LOOK AWAY	SHOW NECK	WALK AWAY	PRESENT	CROUCH	GRIMACE	FLEE
ATTACK																
HITS																
THREATEN																
MOUNT																
APPROACH										3		1	2	1		
STARE								1	1	16	3	2	1			
PACE																
IGNORE								6								
LIPSMACK						9	1		6							
LOOK AWAY					17	82	10			1						
SHOW NECK					7	43										
WALK AWAY				19	32	29	1									
PRESENT				42	15	10										
CROUCH				2	11	3										
GRIMACE			1													
FLEE	2		2	1												

383 TWO-STEP CHAINS

MIDDLE RANK FEMALE

TEN FEMALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	IGNORE	LIPSMACK	LOOK AWAY	SHOW NECK	WALK AWAY	PRESENT	CROUCH	GRIMACE	FLEE
ATTACK																
HITS																1
THREATEN													1		2	2
MOUNT													12			1
APPROACH										6		6	21		1	5
STARE								1	1	22	2	14	10		1	
PACE								1		2		2				
IGNORE						5	2	7								
LIPSMACK						3	2	1	9							
LOOK AWAY					25	66	6			1						
SHOW NECK					7	6										
WALK AWAY				6	24	28	6									
PRESENT			1	27	24	8	1									
CROUCH	4	4	1													
GRIMACE		1	4													
FLEE	6	11	7	2	1											

421 TWO-STEP CHAINS

LOW RANK FEMALE

TEN FEMALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	IGNORE	LIPSMACK	LOOK AWAY	SHOW NECK	WALK AWAY	PRESENT	CROUCH	GRIMACE	FLEE
ATTACK																4
HITS																1
THREATEN														6	21	2
MOUNT													3	3		
APPROACH										4	2	12	5	24	3	
STARE										26	9	8	1	14		
PACE								2		5						
IGNORE					2	2	1									
LIPSMACK						8			9							
LOOK AWAY					41	41										
SHOW NECK					21	42										
WALK AWAY					38	7										
PRESENT					11	5										
CROUCH				1	25	9										
GRIMACE			3													
FLEE	1		3		1											

428 TWO-STEP CHAINS

THREE FEMALES COMBINED

TEN FEMALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	IGNORE	LIPSMACK	LOOK AWAY	SHOW NECK	WALK AWAY	PRESENT	CROUCH	GRIMACE	FLEE
ATTACK																4
HITS																2
THREATEN													1	6	23	4
MOUNT													15	3		1
APPROACH										13	2	19	28	25	4	5
STARE								2	2	64	14	24	12	14	1	
PACE								3		7						
IGNORE					2	7	3	13								
LIPSMACK						20	3	1	24							
LOOK AWAY					83	189	18			2						
SHOW NECK					35	91										
WALK AWAY				25	94	64	7									
PRESENT			1	69	50	23	1									
CROUCH	4	4	1	3	36	12										
GRIMACE		1	8													
FLEE	9	11	2	3	2											

1037 TWO-STEP CHAINS

FIGURE 2. Frequency distribution of two-step chains occurring between 10 females and top-, middle-, and low-rank females. (Lower-right matrix shows combined frequency distribution.)

$$z_c = \frac{y_1 - y_2}{p_2 - p_1},$$

where z_c = a category mean value measured in standard deviation units, y_1 = ordinate in a unit normal distribution of a given category taken as the lower limit of that category, y_2 = ordinate at upper limit of the same category, and $p_2 - p_1$ = proportion of judgments falling in a given category.

In practice, the frequency matrix was converted to a proportion matrix and the y ordinate for each proportion was taken from normal curve tables. These ordinates were the y_2 values for the categories in whose row they appeared and also served as

TOP RANK MALE

TEN FEMALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	BRANCH SHAKE	IGNORE	LIPSMACK	LOOK AWAY	SHOW NECK	WALK AWAY	PRESENT	CROUCH	GRIMACE	FLEE
ATTACK																	
HITS																	
THREATEN																	
MOUNT													2		3		
APPROACH											2		3		3		
STARE									1		21			3	6		
PACE																	
BRANCH SHAKE											1						
IGNORE						3		1	8								
LIPSMACK						1											
LOOK AWAY					2	96	10	17			5						
SHOW NECK						19											
WALK AWAY					24	8	2	6									
PRESENT				1	1	11											
CROUCH				10	16	24		2									
GRIMACE		3	50			2											
FLEE	41	1	3	3	1												

416 TWO-STEP CHAINS

MIDDLE RANK MALE

TEN FEMALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	BRANCH SHAKE	IGNORE	LIPSMACK	LOOK AWAY	SHOW NECK	WALK AWAY	PRESENT	CROUCH	GRIMACE	FLEE
ATTACK																	
HITS																	
THREATEN																	
MOUNT																	
APPROACH											2	5	7	4	16		
STARE									5		24	21	1	1	18		
PACE																	
BRANCH SHAKE									1								
IGNORE						5			24	1	4						
LIPSMACK									8		3						
LOOK AWAY						31	2		8		10						
SHOW NECK						12											
WALK AWAY				1	10	6											
PRESENT				16		10											
CROUCH				2	2	4											
GRIMACE			1														
FLEE																	

265 TWO-STEP CHAINS

LOW RANK MALE

TEN FEMALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	BRANCH SHAKE	IGNORE	LIPSMACK	LOOK AWAY	SHOW NECK	WALK AWAY	PRESENT	CROUCH	GRIMACE	FLEE
ATTACK																	
HITS																	
THREATEN																	
MOUNT																	
APPROACH											1	8	19	2	15		
STARE									9		12	13		2	17		
PACE									1								
BRANCH SHAKE																	
IGNORE							3	2	23	4	1						
LIPSMACK							4	6	11		2						
LOOK AWAY					1	56	5		6		3						
SHOW NECK					1	11											
WALK AWAY					40	6											
PRESENT				16	6	15											
CROUCH			1		4	16											
GRIMACE			6														
FLEE	2	2															

356 TWO-STEP CHAINS

THREE MALES COMBINED

TEN FEMALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	BRANCH SHAKE	IGNORE	LIPSMACK	LOOK AWAY	SHOW NECK	WALK AWAY	PRESENT	CROUCH	GRIMACE	FLEE
ATTACK																	
HITS																	
THREATEN																	
MOUNT													2		3		
APPROACH											5	13	29	6	34		
STARE									15		57	34	1	6	41		
PACE									1								
BRANCH SHAKE											1						
IGNORE						8	3	3	55	5	5						
LIPSMACK						1	4	6	19		5						
LOOK AWAY					3	183	17	17	14		18						
SHOW NECK					1	42											
WALK AWAY				2	74	20	2	10									
PRESENT				42	7	36											
CROUCH			1	2	22	44		2									
GRIMACE		3	57			2											
FLEE	43	3	3	3	1												

1232 TWO-STEP CHAINS

FIGURE 3. Frequency distribution of two-step chains occurring between 10 females and top-, middle-, and low-rank males. (Lower-right matrix shows combined frequency distribution.)

the y_1 values for the categories in the row ranked immediately ahead. Using the above formula, an estimated category mean was obtained for a given category row from every data point in that row. All estimates of means for a given category were then summed and a mean of means derived. The difference between this overall mean for a given category and the mean of its neighboring category was defined as the final scale interval distance between these two categories. Such computations for all categories produced an interval scale of the response categories of the "judge" monkeys. It was then necessary to find the category scale intervals for the stimulus behavior categories produced by the index monkeys.

TOP RANK FEMALE

TEN MALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	BRANCH SHAKE	IGNORE	LIPSMACK	LOOK AWAY	SHOW NECK	WALK AWAY	PRESENT	CROUCH	GRIMACE	FLEE
ATTACK																	3
HITS																	8
THREATEN															1	15	16
MOUNT													4	34	2		1
APPROACH												1	43	23	5	3	1
STARE									1	2	81	6	22	38	7	3	
PACE											16		3				
BRANCH SHAKE																	
IGNORE						2			5					2			
LIPSMACK						1	1										
LOOK AWAY					11	38											
SHOW NECK					2	1											
WALK AWAY					20	5											
PRESENT				11	14	5											
CROUCH					3	1											
GRIMACE		1	2														
FLEE	2		2		1												

469 TWO-STEP CHAINS

MIDDLE RANK FEMALE

TEN MALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	BRANCH SHAKE	IGNORE	LIPSMACK	LOOK AWAY	SHOW NECK	WALK AWAY	PRESENT	CROUCH	GRIMACE	FLEE
ATTACK																	5
HITS																	
THREATEN																26	1
MOUNT													3	7	5		
APPROACH											7	5	35	12	9	1	
STARE									6	1	70	34	2	7	6		
PACE									5		7		3				
BRANCH SHAKE									5		12		2				
IGNORE						4		1	17		2						
LIPSMACK																	
LOOK AWAY					2	49			2		4						
SHOW NECK					6	1											
WALK AWAY					7	5											
PRESENT					3	4											
CROUCH					2	9											
GRIMACE			1														
FLEE	3		2														

400 TWO-STEP CHAINS

LOW RANK FEMALE

TEN MALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	BRANCH SHAKE	IGNORE	LIPSMACK	LOOK AWAY	SHOW NECK	WALK AWAY	PRESENT	CROUCH	GRIMACE	FLEE
ATTACK																	25
HITS																	6
THREATEN																80	18
MOUNT																	
APPROACH											4		47	4	5	2	
STARE										2	54	13	49	11	4	6	
PACE									1		10		6				
BRANCH SHAKE																	
IGNORE						1			3								
LIPSMACK						2											
LOOK AWAY					11	34											
SHOW NECK					7	6											
WALK AWAY					7	1											
PRESENT					6	2											
CROUCH					19	5											
GRIMACE																	
FLEE																	

453 TWO-STEP CHAINS

THREE FEMALES COMBINED

TEN MALES	ATTACK	HITS	THREATEN	MOUNT	APPROACH	STARE	PACE	BRANCH SHAKE	IGNORE	LIPSMACK	LOOK AWAY	SHOW NECK	WALK AWAY	PRESENT	CROUCH	GRIMACE	FLEE
ATTACK																	33
HITS																	14
THREATEN															3	121	35
MOUNT													7	41	7		1
APPROACH											11	6	125	39	19	6	1
STARE									7	5	205	53	73	56	17	9	
PACE									6		33		12				
BRANCH SHAKE									5		12		2				
IGNORE						7		1	25		2			2			
LIPSMACK						3	1										
LOOK AWAY					24	121			2		4						
SHOW NECK					15	8											
WALK AWAY					34	11											
PRESENT				11	23	11											
CROUCH					24	15											
GRIMACE		1	3														
FLEE	5		4		1												

1322 TWO-STEP CHAINS

FIGURE 4. Frequency distribution of two-step chains occurring between 10 males and top-, middle-, and low-rank females. (Lower-right matrix shows combined frequency distribution.)

These scales were found by the simple equation

$$S_b = \Sigma p_x c_x,$$

where S_b is the scale value of a given stimulus category, p_x = proportion of stimuli in Response Category x, and c_x = scale value of Response Category x. The three resulting "raw" interval scales are given in Table 4.

At this point the scales had no logical zero point and each had a different standard deviation. They needed to be combined into a final common scale with a zero point and standard deviation of 2.0. As previously noted, a basic hypothesis of this study was that the values of submissive responses were equal to and opposite the values of stimulus categories. One might then arbitrarily add to each scale an appropriate constant that would give a value of zero to some mid-scale category. One could set this zero point more accurately, however, by using the 2,829 long-step chains obtained from the observation sessions. Analysis of behavioral interaction had earlier led to the hypothesis that long-step chains, as well as two-step chains, were terminated only when the series of responses by the submissive monkey were equal to and opposite the series of stimulus categories given by the dominant. If this hypothesis was correct, and if the intervals between categories derived from the two-step chain data were correct, then one should be able to substitute these interval values into the long-step chains and demonstrate that the sums of scale values of categories in all long-step chains in a particular animal grouping were distributed about a single value. When this single value was made to approximate zero by adding an appropriate constant to the scale, the scale itself would have a correct zero point. Accordingly, the category interval values for each scale were applied to the long-step chains obtained from that particular grouping. The sums of the chains were plotted and the zero point of the scale repeatedly adjusted until the greatest proportion of the frequency distribution of these sums deviated between + 2.0 and − 2.0 and the peak frequency was between 0 and −.5. Figure 5 (A, B, and C) shows the final distribution of all long-step chains about zero for each pair-type grouping when the appropriate zero point for the respective scale had been derived. The three scales with appropriate zero points were then converted by linear transformation to scales with a standard deviation of 2.0 (Table 5). To obtain a final scale, the three scales were averaged, giving double weight to the scale

TABLE 4

RAW SCALE INTERVALS DERIVED FROM TWO-STEP CHAINS

Categories	Monkey pairs: Male	Female	Male–Female
Attack	10.13	10.88	14.44
Hit	10.07	10.85	13.26
Threaten	9.09	10.62	13.23
Mount	8.11	8.71	9.65
Approach	7.94	7.78	9.16
Stare	6.96	6.98	8.13
Pace	6.08	6.61	7.12
Branch shake			6.91
Ignore	4.08	4.87	5.73
Lipsmack	3.25	4.52	5.00
Look away	2.83	2.89	4.59
Show neck		2.73	4.01
Walk away	2.03	2.28	3.48
Crouch		1.52	3.45
Present	1.35	1.55	3.23
Grimace	1.33	.17	1.15
Flee	0.00	0.00	0.00
SD	3.38	3.67	4.11

TABLE 5

DERIVATION OF FINAL SCALE FROM NORMALIZED PAIR SCALES

Categories	Monkey pairs: Male	Female	Male–Female	*M*	Final scale
Attack	3.35	3.09	3.97	3.59	3.25
Hit	3.35	3.09	3.39	3.30	3.00
Threaten	2.75	2.97	3.37	3.12	2.85
Mount	2.18	1.94	1.62	1.84	1.70
Approach	2.08	1.43	1.38	1.57	1.45
Stare	1.50	1.00	.88	1.07	.95
Pace	.98	.80	.38	.64	.60
Branch shake			.28	.28	.25
Ignore	−.20	−.15	−.30	−.24	−.25
Lipsmack	−.70	−.33	−.65	−.58	−.55
Look away	−.95	−1.21	−.86	−.97	−.90
Show neck		−1.29	−1.14	−1.19	−1.10
Walk away	−1.40	−1.54	−1.44	−1.46	−1.35
Crouch		−1.89	−1.42	−1.58	−1.45
Present	−1.82	−1.93	−1.53	−1.70	−1.55
Grimace	−1.82	−2.67	−2.53	−2.39	−2.20
Flee	−2.60	−2.77	−3.10	−2.89	−2.65

Note. Pearson correlation coefficients for the normalized scales: *r* male vs. female = .79, *r* female vs. male–female = .99, *r* male vs. male–female = .96.

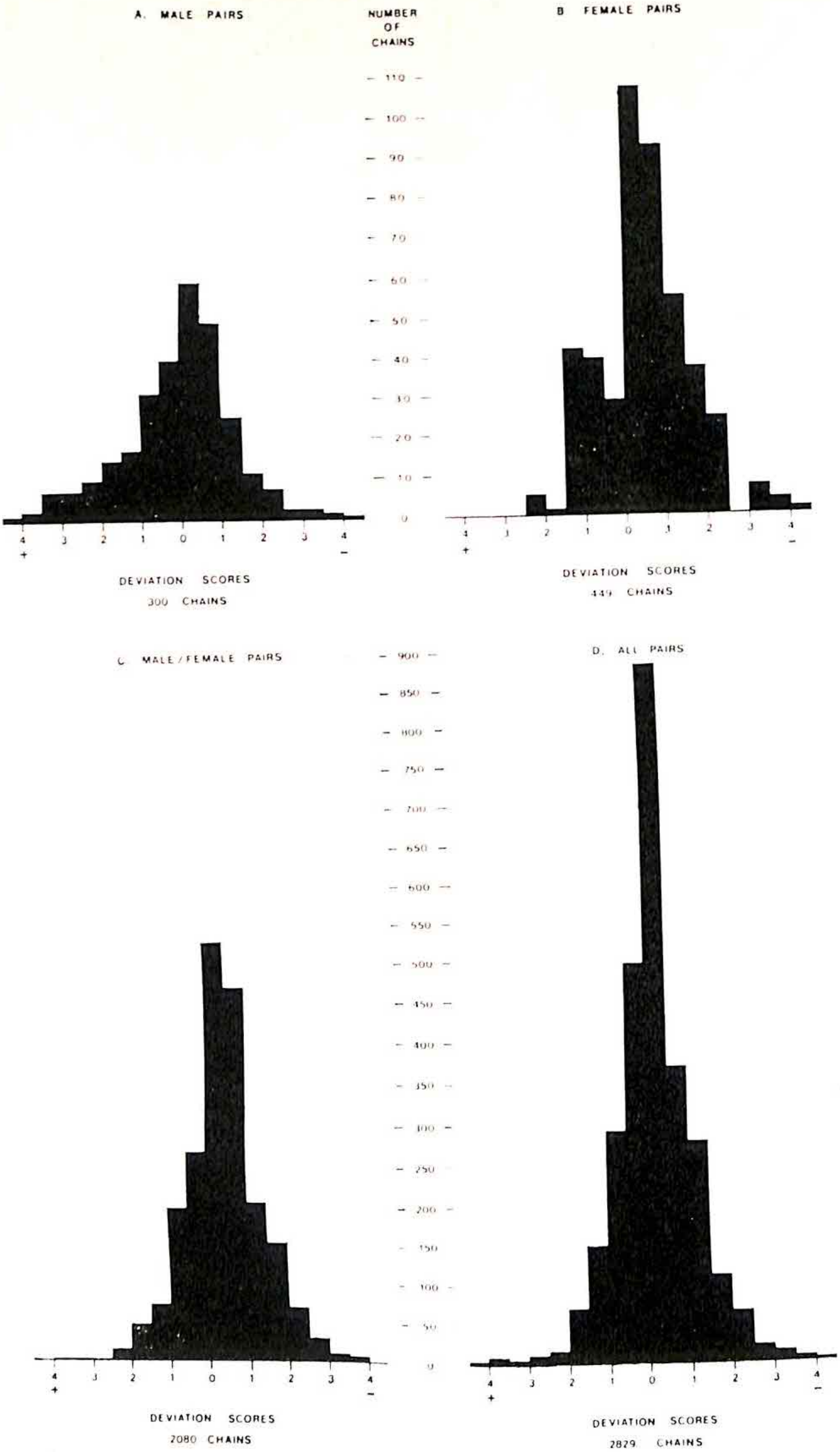

FIGURE 5. A–C: Distribution of long-step chain scores about zero, obtained by summing derived scale values for each long-step chain found in a given pairing type. (These long-chain distributions are shown for male, female, and male–female pair groups when appropriate zero point for the given scale had been derived.) D: Distribution of scores about zero obtained by summing final scale values for all categories in all 2,829 long-step chains from all pair groups.

derived from the pooled male–female and female–male groupings. The resulting mean scale was again adjusted to yield a standard deviation of 2.0, and interval values were adjusted to the nearest .05. The internal consistency of the final scale (shown in Table 5) was checked by applying it to all 2,829 long-step chains and plotting the frequency distribution of deviation scores (Figure 5D). Using this scale, the deviation scores of 2,663 of 2,829 long-step chains (94%) fall between +2.0 and −2.0, with 881 (31%) of all chains between 0 and −.5. The Pearson product-moment correlation coefficient, calculated for the three scales as a check on the reliability of the scaling procedure, ranged from .79 to .99 (Table 5).

The final scale could now be used to examine the time course of changing communication between monkeys as their relationship developed. Continued observation of these adult monkey pairs revealed that once a stable dominance–submission relationship was established, increasingly less communication of a dominance–submission nature was needed to maintain it. This change was analyzed by summing the scale values of all categories produced by each animal at 45-sec intervals. This summed value was then used to place the monkey on the dominance–submission dimension. The 45-sec time unit was used because that was inherent in the method of deriving the scale. Long-step chains and two-step chains seldom take more than 15 sec to perform and, as defined, they have both preceding and following 15-sec intervals of no activity. Therefore, the time unit for this scale was 45 sec, and this unit or multiples of it must be used to maintain scale accuracy when plotting the temporal course of pair relationships. It was assumed that the summed value of behavior categories at these intervals reflected each monkey's assessment of where he stood in relation to the other monkey on the dominance–submission dimension.

Figures 6–9 show typical relationships established for four types of rhesus pairings. Figure 6 demonstrates the relationship between the alpha and beta males of the male group. The top graph plots each monkey on the dominance–submission dimension and shows that for the first 18 min both animals demonstrated marked dominant behavior; in fact, they engaged in a fight lasting almost 2 min, after which the beta male gave up and began showing submissive behavior. From this point the plot of the submissive animal's behavior is virtually a mirror image to that of the dominant. The bottom graph represents the arithmetic sum of the scaled behavior of both animals and plots the deviation of one animal's score against the other. It clearly distinguishes between the first 18 min of the pairing when the tension level and fighting were high, and the period after the last fight when a dominance–submission relationship was set up. The upper graphs of Figures 7 and 8 show the rapid onset of a dominance–submission relationship for a high-rank and a low-rank male, and a high- and a middle-rank female, respectively. Both graphs demonstrate a gradual decrease in intensity and frequency of dominance and submission categories, and animals in both pairs appeared to act in such a way that their behavior approached zero or neutrality on the dominance–submission dimension. This process was accelerated when the alpha male was paired with an estrous adult female (Figure 9). In all three pairings, the animals reached a point at which positive advances leading to grooming took place. The lower graphs in Figures 7, 8, and 9 show that the momentary behavior category sums mildly fluctuated about zero. Here, reduction in frequency of dominance categories by one monkey was clearly matched by a corresponding decrease in submission categories by its pair mate. These graphs generally typify the temporal course of social relations in all adult pairs observed.

As the dominance–submission relationship was maintained at a lowered intensity, the frequency of long-step chains often increased which, this time, appeared to bring the animals together. The end result of a series of these long-step chains was the initiation of mounting and presenting episodes accompanied by grooming. In male–male pairings these mounting–presenting sequences often occurred reciprocally, with the dominant male initiating a sequence by presenting. In male–female pairings when the females were in estrus, sexual mounting and presenting sequences generally led to intromission and ejaculation by the male. In female–female pairings, mounting–presenting sequences were infrequent and appeared to be replaced by sequences in which one animal would approach the other and crouch or show neck, apparently inviting grooming.

The long-step chains which preceded all of these events must have contributed to establishing these positive bonds. Since the chains were made up of the same behavior categories which could be aligned along the aggression–fear continuum implicit in the interval scale, there must have been something different in the internal content of the chains which decreased communication of aggression or fear and enhanced social closeness. Similarly, since the behavior of each monkey approached a low level on the dominance–submission dimension just before grooming or other positive

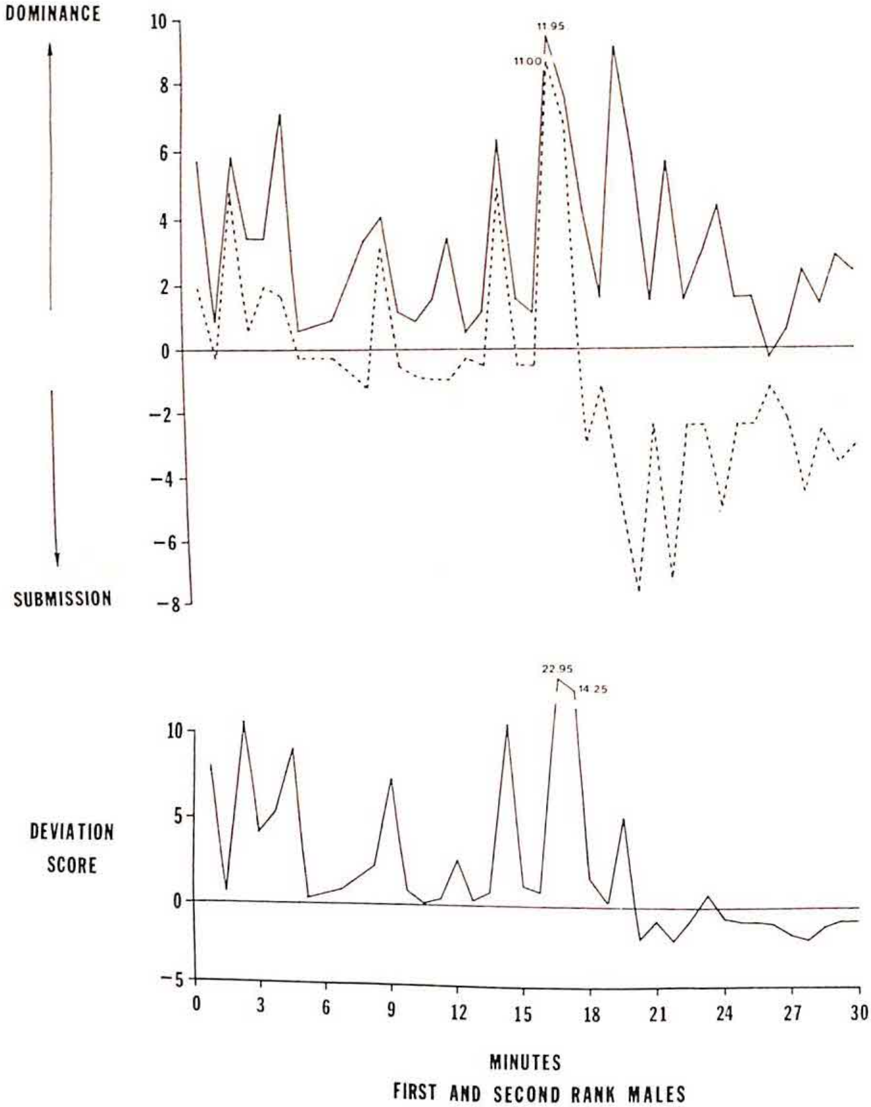

FIGURE. 6. Typical relationships established for first- and second-rank males. (In the top graph of each pairing the summed categories produced by the dominant monkey are plotted at 45-sec intervals on the dominance–submission dimension as a solid line, those of the submissive monkey as a dotted line. Black bars indicate mutual grooming bouts. The deviation score in the lower graph of each pairing shows the arithmetic sum of behavior categories of both animals at 45-sec intervals and reflects the degree to which the behavior of the submissive animal is equal and opposite in sign [appropriately submissive] to that of the dominant.)

bonding activity, there must be a zone of "friendliness" in this dimension that could be quantitatively defined. Thus, the long-step chains were collected and analyzed both to determine the broad motivational states that they appeared to communicate and to aid in defining the zone limits on the dominance–submission dimension within which a monkey might communicate positive affect. In the following analysis all long-step chains were classified using the previously derived rating scale to determine the internal makeup which made some chains appear to express dominance or submission and others, friendliness. The summed interval scale values for different types of chains were then used to define a friendliness zone on the dominance–submission dimension.

It should be noted that a basic hypothesis of this study was that a long-step chain, as well as a two-step chain, did not usually terminate until the behavior sequence for one animal in the chain became the quantitative reciprocal of the sequence of the other. As previously shown, this appeared to be statistically true in that the categories of all chains could be summed and their sums grouped around zero. This implied that what was important in comparing one chain with another was not the internal serial order of categories making up the chain, but the sum of categories made by one animal versus

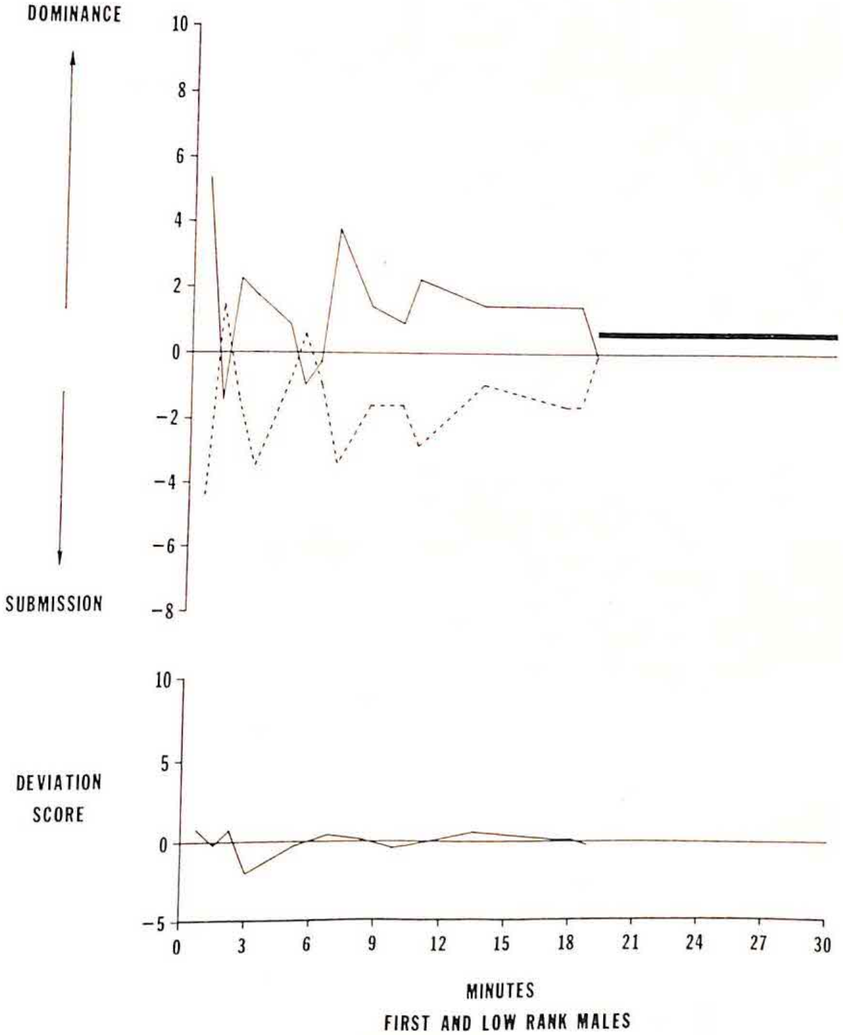

FIGURE 7. Typical relationships established for first- and low-rank males. (See Figure 6 caption for explanation of upper and lower graphs.)

the sum of those made by the other. Thus, regardless of serial order, chains that had identical category components were considered identical. Category components performed by one animal were considered as a group making up one half of a chain, separate from those performed by the other animal. These half-chains were then examined and classified according to where their category components fell on the interval scale.

Previous studies have commented on the fact that grooming appears to induce relaxation, decrease aggression, and promote social affinity (Altmann, 1962; Hinde & Rowell, 1962; Sade, 1965). Therefore, as a second means of determining the zone of friendliness on the dominance–submission dimension, all "bouts" of grooming in all pairs were examined. The definition of a *bout* is taken from Sade (1965) in which the groomer slides, scrapes, and picks at the fur of the other monkey but these motor patterns may be interrupted during a single bout by sitting, looking around, scratching and, frequently in this study, mount–present two-step chains. Long-step chains occurring during grooming bouts or that preceded or followed such bouts by less than 1.5 min were separately tabulated and arbitrarily defined as signifying friendliness.

A total of 2,829 long-step chains occurred

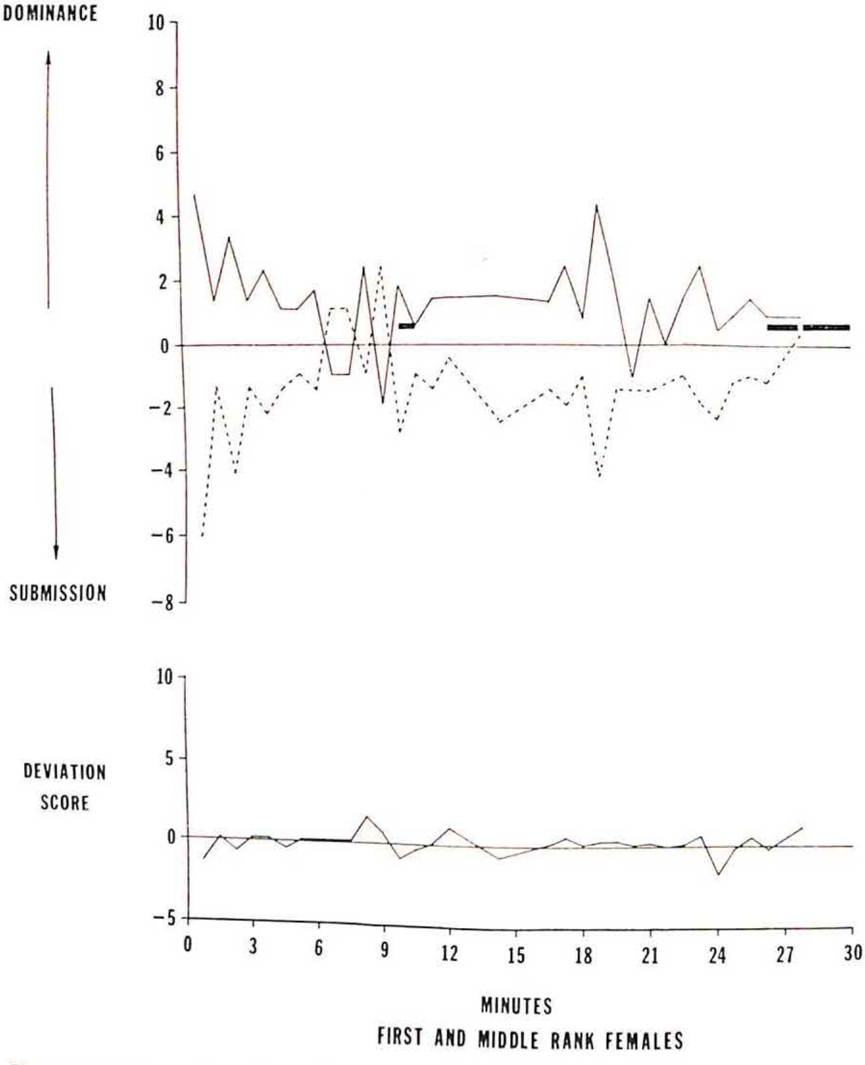

FIGURE 8. Typical relationships established for first- and middle-rank females. (See Figure 6 caption for explanation of upper and lower graphs.)

in 872 types of sequence. When the categories of one animal were grouped together in a half-chain separate from those performed by the other animal, all half-chains could be classified in one of four groups. (a) Dominant: The categories in the half-chain were all dominant, that is, had positive scale values. (b) Subdominant: The half-chain consisted of very dominant categories and a single, mildly submissive category (a category with a negative scale value). The sum of these categories always exceeded + 1.0 scale unit. (c) Friendly: The half-chain consisted of an even mixture of dominant and submissive categories. The set of sums for such chains had a mean of zero. (d) Submissive: The categories in the half-chain were all submissive ones, that is, had negative scale values.

These half-chains were found to occur as five combinations, or classes, of long-step chains. (a) Dominant–subdominant: The categories of the dominant pair mate were all very dominant; those of the submissive pair mate were also very dominant but included a submissive category, for example, hit, threat, pace–threat, pace, look away. (b) Dominant–submissive: The categories of the dominant monkey were all dominant, those of the submissive monkey all submissive, for example, threat, approach–grimace, walk away. (c) Dominant–friendly: The categories of the

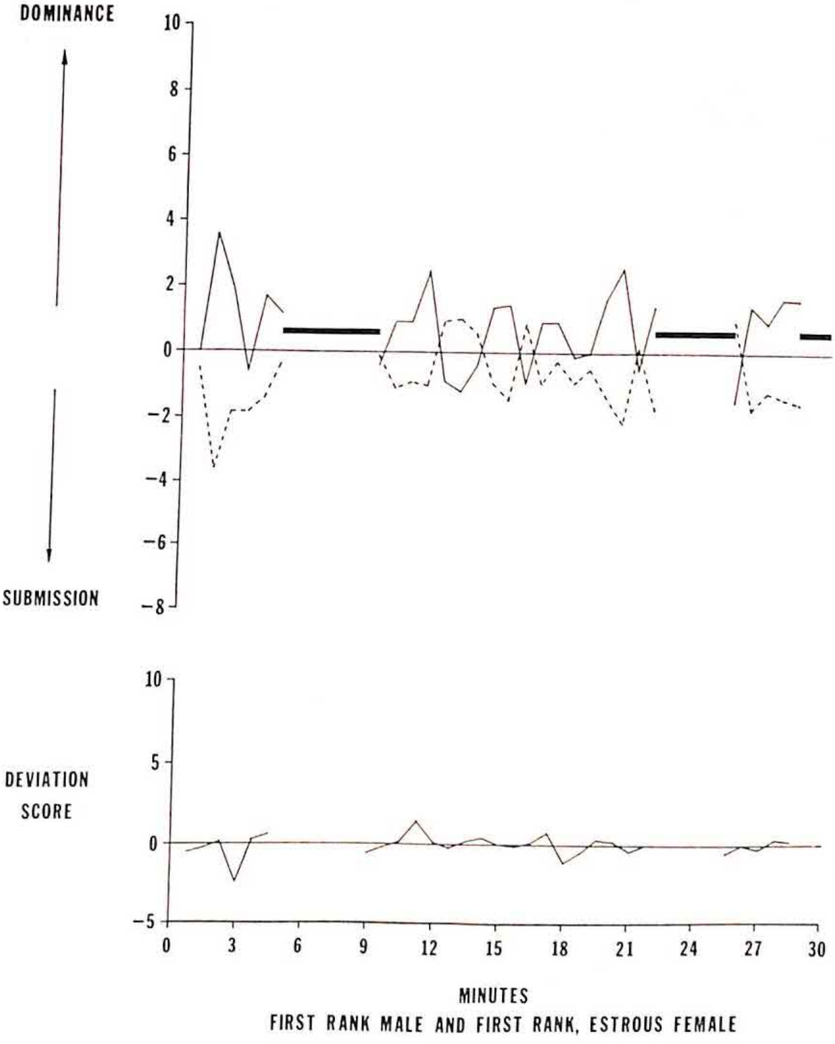

FIGURE 9. Typical relationships established for first-rank male and first-rank, estrous female. (See Figure 6 caption for explanation of upper and lower graphs.)

TABLE 6

Classification of Frequently Observed Behavioral Chains

Dominant behavior	Submissive behavior	Monkey pairings				
		m/m	f/f	f/m	m/f	Total
Dominant/submissive chains[a]						
attack	grimace, flee	—	—	6	3	9
attack, threat	grimace, flee	—	—	3	6	9
threat	look away, walk away	—	—	6	—	6
threat	lipsmack, look away, walk away	—	7	—	1	8
threat	lipsmack, grimace	—	2	1	7	10
threat	walk away, grimace	7	2	4	1	14
threat, approach	walk away, grimace	1	—	9	13	23
threat, approach	flee	1	2	7	2	12
threat, approach	grimace, flee	8	1	17	2	28
threat, stare	walk away, grimace	1	—	7	3	11
threat, pace	walk away, grimace	7	—	2	3	12
threat, pace	grimace, flee	7	—	—	1	8
threat, lipsmack	look away, walk away	—	—	7	—	7
threat, approach stare	grimace, flee	1	—	3	2	6
threat, approach, stare, pace	grimace, flee	—	—	3	5	8
mount	lipsmack, present	20	—	4	—	24
mount	walk away, present	6	2	—	—	8
mount, approach	walk away, present	16	19	1	2	38
mount, approach	crouch, present	—	—	2	5	7
mount, approach	present, present	—	11	8	7	26
mount, approach, pace	present, present	—	5	2	1	8
approach	lipsmack, look away	—	8	1	4	13
approach	lipsmack, present	—	1	7	—	8
approach, stare	look away, walk away	—	4	5	—	9
approach, stare	look away, crouch	—	1	8	2	11
approach, stare	walk away, present	—	5	1	3	9
approach, stare	grimace	—	1	3	5	9
approach, stare	walk away, grimace	1	8	5	7	21
approach, stare	flee	5	4	6	2	17
approach, stare	lipsmack, look away, walk away	—	—	4	4	8
approach, stare, pace	walk away, grimace	—	—	8	9	17
stare	lipsmack, look away	—	12	30	60	102
stare	lipsmack, show neck	—	4	3	6	13
stare	lipsmack, crouch	—	3	1	2	6
stare, pace	grimace	1	—	5	1	7
stare, pace	walk away, grimace	9	—	3	1	13
Dominant/friendly chains[b]						
mount	approach, present	4	13	—	—	17
mount, stare	approach, present	6	1	—	—	7
stare	approach, look away	3	13	17	5	38
stare	approach, show neck	—	11	—	—	11
stare	approach, walk away	—	7	19	2	28
stare	approach, crouch	—	6	5	1	12
stare	approach, present	1	11	17	1	30
stare	approach, grimace	—	—	6	—	6
branch shake	stare, look away	1	—	2	15	18
Friendly/submissive chains[c]						
mount, lipsmack	present	1	—	4	1	6
mount, lipsmack	lipsmack, present	25	—	—	—	25
approach, walk away	look away	—	1	4	1	6
approach, walk away	present	—	5	1	—	6
approach, crouch	look away	—	7	2	9	18
approach, present	look away	5	4	2	11	22
approach, stare, lipsmack	look away, walk away	—	2	10	—	12
approach, stare, pace, lipsmack	look away, walk away	—	—	3	5	8
stare, lipsmack	look away	—	1	20	16	37
stare, lipsmack	walk away	—	—	3	4	7
stare, lipsmack	lipsmack, look away	—	—	1	8	9
stare, lipsmack	lipsmack, walk away	—	—	6	—	6
stare, present	look away	—	1	4	2	7
Friendly/friendly chains[d]						
mount, stare	approach, walk away, present	—	9	—	1	10
mount, look away	approach, walk away, present	—	11	—	1	12
mount, walk away	approach, present	3	1	4	5	13
mount, stare, lipsmack	approach, look away present	3	5	—	—	8
mount, stare, lipsmack, look away	approach, present	—	—	6	1	7
mount, stare, lipsmack, crouch	approach, stare, present, present	—	—	—	7	7
mount, lipsmack, look away, show neck	stare, stare, lipsmack, present	—	—	—	17	17
mount, approach, pace, lipsmack, lipsmack, walk away	stare, present, present	—	—	6	—	6
approach, look away	ignore	—	3	8	8	19
approach, look away	stare, lipsmack	—	—	5	1	6
approach, look away	stare, look away	—	6	25	20	51
approach, look away	stare, walk away	—	8	13	1	22
approach, look away	stare, crouch	—	5	4	2	11
approach, look away	stare, present	1	2	3	2	8
approach, walk away	ignore	—	—	22	18	40
approach, walk away	stare, lipsmack	—	2	15	8	25
approach, walk away	stare, look away	—	4	8	21	33
approach, show neck	stare, look away	—	4	1	12	17
approach, show neck	stare, crouch	—	—	1	5	6
approach, crouch	ignore	—	3	—	12	15
approach, crouch	stare, lipsmack	—	2	7	—	9
approach, crouch	stare, look away	—	—	6	26	32
approach, crouch	stare, walk away	—	7	1	1	9
approach, present	ignore	—	3	7	11	21
approach, present	stare, look away	—	18	54	69	141
approach, present	stare, show neck	—	1	2	6	9
approach, present	stare, walk away	—	11	7	13	31
approach, present	stare, crouch	—	1	—	20	21
approach, present	stare, present	—	3	10	4	17
approach, lipsmack, crouch	stare, look away	—	7	—	2	9
approach, lipsmack, present	stare, look away	—	—	4	2	6
approach, pace, lipsmack, present	stare, look away, look away	—	—	6	—	6
approach, stare, pace, lipsmack, walk away	stare, lipsmack, walk away	—	—	—	8	8
stare, lipsmack	stare, lipsmack	—	—	1	6	7
stare, lipsmack	stare, look away	—	2	10	1	13
stare, look away	branch shake, look away	2	—	1	3	6
stare, look away	stare, present	—	—	2	6	8
stare, look away	stare, grimace	—	1	3	4	8
stare, look away	stare, lipsmack, look away	—	—	9	1	10
stare, look away	stare, lipsmack, present	—	—	6	—	6
stare, look away	stare, pace, lipsmack, present	—	—	2	4	6

Note. For each chain the table shows the dominant monkey, then all behaviors of the submissive monkey; 1,598 of 2,829 chains (56%) are shown as produced by 99/372 sequence types (11%); m/m = male-male pairings, f/f = female-female pairings, f/m = female-male pairings, and m/f = male-female pairings.

[a] Thirty-six types occurred more than five times ($n = 545$).

[b] Nine types occurred more than five times ($n = 167$).

[c] Thirteen types occurred more than five times ($n = 170$).

[d] Forty-one types occurred more than five times ($n = 716$).

TABLE 7

Distribution of Long-Step Chains from all Pairings

Chain class	No. of types ($N = 872$)	Total no. ($N = 2,829$)	Distribution of chains across classes				Mean scale value	*SD*
			m–m ($N = 300$)	f–f ($N = 449$)	f–m ($N = 987$)	m–f ($N = 1,093$)		
Dominant–subdominant	6	10	1%	—	.5%	—	3.55 1.44	1.45 .48
Dominant–submissive	219	872	56%	32%	28%	27%	2.81 −3.01	1.35 1.21
Dominant–friendly	70	260	10%	13%	11%	5%	1.46 −.44	1.18 .93
Friendly–submissive	127	324	18%	11%	10%	10%	.71 −1.90	.94 1.06
Friendly–friendly	450	1,363	15%	44%	50.5%	58%	.15 −.33	.84 .88

Note. m = male, f = female.

dominant animal were all dominant; those of the submissive animal were a mixture of dominant and submissive types, for example, approach, stare, pace–stare, lipsmack, walk away. (d) Friendly–submissive: The categories of the dominant animal were a mixture of dominant and submissive, those of the submissive monkey all submissive, for example, approach, present–look away, grimace. (e) Friendly–friendly: The categories of both the dominant and submissive animals were a mixture of dominant and submissive types, for example, approach, present–stare, look away.

The dominant–submissive and friendly–friendly chains occurred most commonly, accounting for 2,235 of the 2,829 chains. Those chains in each class that were observed more than five times are cataloged in Table 6. Only 10 chains were of the dominant–subdominant class (Class 1), none of them occurring more than five times, so Table 6 shows chains only from Classes 2–5. The distribution of all long-step chains across classes, the number of chain sequence types of each class, and the percentage of chains from each class which occurred in a particular type of pairing are summarized in Table 7. Table 7 also shows the mean and standard deviation of the summed scale values for all half-chains in a particular class. Combining all "friendly" half-chains in all classes yielded a mean of −.04 and a standard deviation of 1.05.

Long-step chains cataloged from grooming bouts were grouped by the type of pairing in which they occurred, and results of analysis are shown in Table 8. The scale value for each half of a long-step chain was computed for all chains occurring during such bouts. As can be seen from Table 8, the scale values of various friendly half-chains ranged from a mean of 1.17 to − 1.19 on the dominance–submission dimension.

Discussion

An interval scale has been constructed of 17 categories occurring spontaneously between 120 pairs of adult rhesus monkeys. It should be noted that some categories occurring around the middle of the scale are probably underreported since it is hard to tell if two monkeys are mutually *ignoring* one another, or one is *ignoring* and the other *looking away,* or both are *looking away,* as defined above. Occurrences were recorded only when it seemed obvious that the animal was actively *ignoring,* as in brief breaks in fighting or threat episodes. Since the peak frequencies of *ignore* and *look away* are well localized, however, this underreporting probably has little effect on the final scale values. The infrequent use of the category *head bob* in all pairings leads to speculation that its main function is to get the attention of a monkey some distance away who could not see a smaller physical gesture. *Branch shaking* appeared to be primarily a tension-relieving activity; it occurred before a close relationship had been established between pair mates, was generally done while facing away from the pair mate, and usually caused the pair mate to look rapidly from the performer to the distant spot that the performer was facing. Similarly, the category *pace* was often ignored or the other animal looked away, and one might infer that by itself it communicated heightened tension in the performer that might or might not be directed to the pair mate.

On occasion a submissive monkey used *grimace* as a general response to several dominance categories which elicited a variety of responses from other monkeys. Such biasing of the data by the response of an individual monkey was minimized by using a large number of different pairs of monkeys as subjects and by collecting large numbers of two-step chains. In human psychophysical scaling experiments the experimenter can control the number of presentations of each stimulus to each subject and thus generally insure that every subject is exposed to the same number of stimuli. No such control was possible in this study. However, most pairs soon established a stable dominance–submission relationship and went on to develop evidence of affectional bonds as demonstrated by sexual activity and mutual grooming. Thus, the "responding" monkey in most pairs was exposed to a range of behavioral stimuli, though not a balanced number of them. Hopefully, this lack of control was compensated for by the large number of subjects yielding large data samples.

In constructing the scale, the validity of comparisons across relationships rests on the assumption that the observed social interactions between pairs of monkeys were accurate samples of general social relationships in this species; this same assumption would be needed to use the scale on other captive or wild rhesus populations. Many monkeys who are captive for long periods develop stereotyped motor patterns, apathy, and withdrawal. Though all animals in this study were screened to rule out these deviations, one could not claim that they were representative of the wild population. However, the communicative meaning of their behavior categories is representative of those found in wild rhesus since, being at least 2 years old when captured, they had learned their repertoire in the wild. The scale values were dependent for their derivation on the relationship between pairs of categories—on the communicated meaning of them—and not on an absolute frequency or environmental context. Thus, the communicative value of these categories is the same in either the captive or wild state, so this scale should be useful for studying all rhesus populations.

Almost all the common statistical procedures may be applied to these interval scale values including the mean, standard deviation, Pearson product-moment r and other statistics depending on these values. The coefficient of variation is the only common statistic that cannot be computed as it requires a ratio scale with an absolute zero point. Apart from these considerations, the usefulness of such a scale depends on its empirical validity and consistency as well as the ease with which it brings coherence to data as diverse as discrete behavior categories. There were 872 combinations of categories observed as long-step chains. When the scale category values derived from two-step chains were applied to these long-step chains, the chain scores were distributed

TABLE 8

LONG-STEP CHAINS ASSOCIATED WITH GROOMING BOUTS

Type of pairing	Grooming bouts	Behavioral chains	Mean scale value	
			Dominant animal	Submissive animal
Male–males	47	157	1.17	−1.19
Female–females	102	406	.94	−.96
Male–females	33	314	.31	−.53
Female–males	34	218	.43	−.51
Overall mean			.69	−.78
Range			.31 to 1.17	−.51 to −1.19

about a single modal point (Figure 5). This appeared to be the best test for consistency of the category intervals within the scale. Further empirical validity can be gained by noting that as now derived, those pairs of categories which from observation give peak modal distributions (Figures 1–4) have, as expected, nearly equal and opposite scale values, for example, *mount–present, stare–look away,* or *approach–walk away.*

Several technical points should be mentioned about using this scale. A situation frequently arises in which, for example, one monkey approaches the other, who *presents*; the approaching monkey then *mounts* the other monkey, who continues to *present.* It is important to record the presenting activity twice. These four behaviors would then be a long-step chain: *approach, mount–present, present.* In general, for every behavior that one monkey performs, a response by the partner must also be scored except, of course, for the last behavior in a chain.

There is an unsatisfactory dichotomy in the submissive portion of the scale between *walk away* and *flee* in that these two categories represent extremes of a motor activity continuum and frequently monkeys use another portion of the continuum–trotting away, jumping away, etc. I arbitrarily scored everything short of panicked flight as *walking away.* If the scale were to be applied to animals moving in a larger environment, "middle range" flight categories would need to be defined and inserted. More generally, a factor accounting for varying distances separating communicating monkeys may need to be defined and applied to these scale values when used on widely ranging populations.

When the scale was used to analyze the observed long-step chains, each chain could be placed in one of five classes based on the types of categories seen. Eighty percent of all chains fell in either a dominance–submission class in which the behavior categories of one animal mirrored those of the other and together summed approximately zero, or a friendly–friendly class in which each animal mixed dominant and submissive categories in such a way as to place himself close to zero. The distribution of these two classes of chains was clearly not uniform within a group of pairings and differed across groups. Table 7 shows that 56% of all chains occurring in male–male pairings were of the dominance–submission class and thus involved in establishing or maintaining a dominance–submission relationship, whereas only about one third of long-step chains occurring in other types of pairings were of this class. Conversely, 44 to 58% of chains in the two male–female and the female–female groups were of the friendly–friendly class and thus involved in establishing positive social bonds, whereas only 15% of chains in male–male pairings were devoted to this class.

The dominant-friendly and friendly-submissive classes appeared to be less frequent interactions in which the submissive animal either underreacted or overreacted, respectively, to behavior categories from the dominant monkey. As can be seen in Table 7, in the dominant–friendly class, the dominant half was frequently a single behavior of low ranking value on the scale. A friendly rather than submissive response by the submissive monkey perhaps represented uncertainty as to what his response should have been, or an attempt to "push" the relationship toward increasing positive social bonding. The submissive halves of friendly–submissive chains were, similarly, low levels of submission. This submissive reaction perhaps represented passivity and nonacceptance of a positive social advance, or reflected the immediate social context more than other classes of chains. Eighteen percent of chains in pairings between males were of this friendly–submissive class, compared with 10% of chains in other pairing groups. As previously noted, most behavior in male–male pairings occurred in a dominance–submission context which might increase the probability of overreacting.

The classification clearly demonstrated that any monkey, whether the dominant or the submissive pair mate, communicated friendliness to the other by "mixing" dominant and submissive categories in close temporal sequence within a chain. Such mixing accounted for most of the richness within the social relationship. By mixing categories, either animal could approach the other without causing the other to either attack or flee. The significance of this was clear when one noted that in 1,098 of 1,363 (80%) friendly–friendly chains, one monkey initiated the chain by approaching the other.

As shown in Figures 6–9 immediately preceding the onset of grooming the dominant animal often produced a long-step chain in which submissive categories predominated. If one assumed that long-step chains occurring around the time of grooming must be made up of predominantly friendly classes of chains, then the range of scale values from 1.17 to − 1.19 on the dominance–submission dimension, obtained for chains from all grooming bouts, must roughly define the limits of a zone of "friendliness" on this dimension. One could corroborate and refine this estimate by calculating the mean and standard deviation of summed interval scale values for each half of all chains in all classes. These values are shown in Table 8. The mean value of all friendly half-chains was −.04 with a standard deviation of 1.05. Since, assuming a normal distribution, 95% of all friendly half-chains should be within two standard deviations of this mean, one could define a "zone of friendliness" from + 2.06 to − 2.06 on the dominance–submission dimension which should include almost all half-chains which could be construed as friendly. On the other hand, the mean of the dominant halves of all dominant–submissive chains was 2.81 with a standard deviation of 1.35. Therefore, a series of behavior categories whose overall half-chain value was 1.50 on the dimension could imply almost equally either a dominant or friendly overall attitude to the submissive animal. Indeed, in those cases where the submissive animal gave a friendly response to a dominant type of category from the dominant animal (dominant-friendly class), rather than a submissive response, the mean of sums of these dominant categories was found to be 1.46. One might thus infer that there is a region of ambiguity from about 1.00 to 2.06 on the dimension. Category sequences whose sum values fell in this value range may or may not signify friendliness. For purposes of analysis of pair relationships, then, a zone of friendliness will be defined here as extending from 1.50 to − 1.50 on the dominance–submission dimension. This limit represented 1.5 standard deviations from the mean value of all "friendly" halves of the chains, and thus included 85% of all friendly behavior sequences, while excluding 66% of behavior sequences which could be interpreted as expressing dominance.

Added indicators consistent with these zone limits could be found when one considered the amount of difference between a dominant and a submissive category tolerated by the dominant animal without appearing to elicit further categories on his part. It was obvious that the dominant monkey would tolerate some underresponding to his dominance behavior, but beyond a certain point he would "push" the issue further and make another dominance behavior. If one applied the scale values to the basic data matrices, the greatest tolerated discrepancy, short of causing the dominant animal to initiate another response which changed a two-step to a long-step chain, was found in the boundary categories of a response distribution. For two-step chains, the discrepancies between *mount–walk away, approach–look away, stare–ignore,* and *pace–ignore* were .35, .55, .70 and .45, respectively. Roughly, then, the tolerated discrepancy appeared to be about .50. Similar examination of the sums of all long-step chains as they were distributed about zero in Figure 5D permits an estimate of this tolerance from the distribution of the positive portion of deviation scores. Here, 75% of chains deviated by 1.00 or less and 90% of all chains were within 1.5 scale units of zero. One might infer that rhesus monkeys have difficulty discriminating aggression expressed by behavior less than 1.0 unit from zero. As one moves further up the scale this discriminal difference probably gets smaller. Tolerance of this magnitude might explain the obvious gap in the scale from zero to the first really dominant, pair-mate–directed behavior category, *stare.* The intervening categories, *branch shake* and *pace,* as previously noted, appear to communicate tension and alertness, which might or might not be directed to the other monkey. Finally, the deviation score graphs of individual pairings as exemplified in Figures 6–9 show that deviations above zero seldom exceed 1.0 to 1.5 units. One can thus infer that the dominant animal tolerated behavior category sequences on the part of the submissive animal which deviated by as much as 1.5 units on the dominance–submission dimension without arousal of further dominance responses. There thus appeared to be a zone from 0 to about 1.5 scale units in which a submissive animal could behave in a friendly manner, mixing dominant and submissive categories, without being misinterpreted.

The extent of the friendly zone on the submissive side of the dimension depended on its extent on the dominant side. As noted above, monkeys maximize their gains and minimize their losses: Submissive monkeys respond to a friendly sequence with as little submission as possible. On the other hand, the values of submissive categories in the friendly "mixed" response of the submissive animal had to equal the values of dominance in the dominant animal's friendly "mix." Therefore, the negative limit of the friendly zone had to be as great as the positive extent.

A zone of friendliness from 1.5 to −1.5 on the dominance–submission dimension could be applied to analysis of individual pair relationships as in Figure 10. This figure shows that the temporal course of a pair relationship could now be broken into segments based on the predominant occurrence of a particular class of long-step chains. It shows the relationship, as it was established over one 90-min session, between an 11.3-kg dominant male about 8 years old and a 10.6-kg subdominant male about 6 years old, both of which had been in captivity less than 3 months. The behavioral responses of this pair were not obviously different from those of the population of longer-term captive monkeys used to establish the interval scale. Following an initial 2.5-min period of fight-

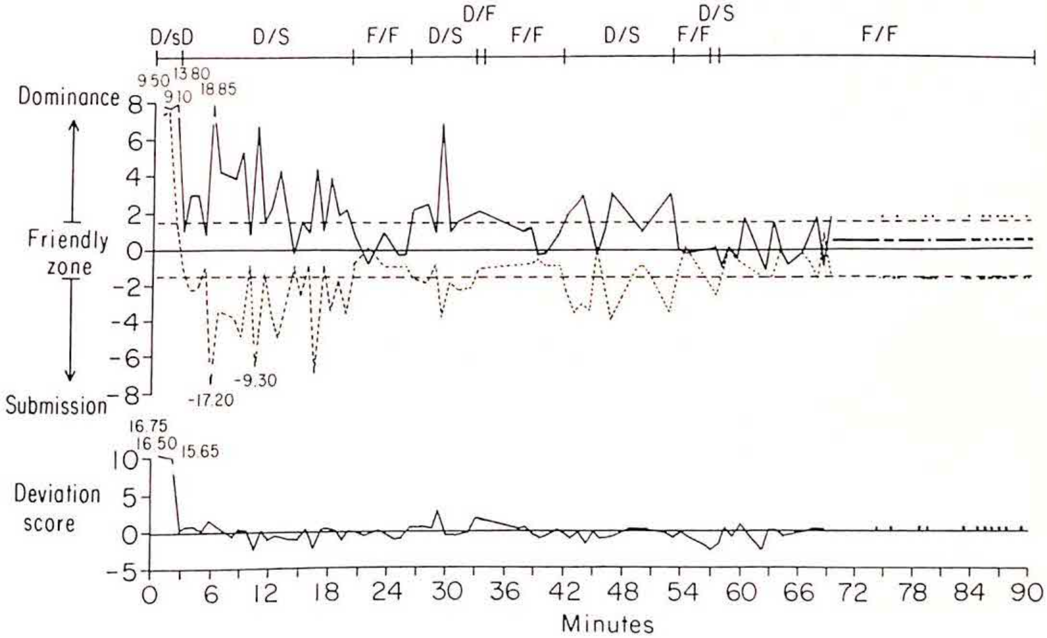

FIGURE 10. Upper graph: Summed behavior categories of the dominant animal are plotted at 45-sec intervals as a solid line on the dominance–submission dimension, those of the submissive monkey as a dotted line. (Temporal sequences of classes of chains: D/SD = dominant/subdominant, D/S = dominant/submissive, F/F = friendly/friendly, D/F = dominant/friendly, F/S = friendly/submissive. Black bars indicate grooming bouts. Single points plotted from 72 min indicate mount–present sequences.) Lower graph: Deviation score is the arithmetic sum of behavior categories of both monkeys at 45-sec intervals and reflects the degree to which the behavior of the submissive monkey is equal and opposite in sign (appropriately submissive) to the dominant.

ing combined with chains of the dominant–subdominant class, the dominance–submission relationship was established and the next 17 min were spent in intense but slowly waning chains of the dominance–submission class. A short period of the friendly–friendly class of chains was followed by a short, intense period of the dominance–submission class. This sequence was repeated again but the last period of chains of the dominance–submission class was increasingly interspersed with intervals of no interaction. The last 40 min of the 90-min session were spent on the friendly–friendly class of chains leading to grooming. The 20-min bout of grooming was repeatedly punctuated with brief mount–present two-step chains characteristic of grooming episodes between males.

The behavior category *mount* has a scale value of 1.70, falling in the ambiguous region of friendliness and dominance on the dominance–submission dimension, so it seems possible that the prevalence of this behavior in all pairings may have a dual use. Many submissive monkeys *grimace* and *flee* when the dominant monkey first attempts to mount them, clearly demonstrating the dominance value of such mounting attempts. However, other studies have commented on the value of the mount–present activity in cementing a positive social bond (Altmann, 1962; Carpenter, 1964; Chance, 1956; Hinde & Rowell, 1962). In this study no dominant animal showed an attack, a hit, or a threat to the pair mate once a series of mountings had occurred between pair mates. Mounting may thus be a means by which an animal engages in friendly social activity while maintaining his "prestige" within the group, as defined by Carpenter (1964): "the perseveration of the effects of previous conditioning of an individual and of the reactions of other individuals to him without his showing behavior which would secure his dominance status" (p. 350).

This catalog of long-step chains also permits examination of frequently used category combinations. One common category sequence deserves comment. The very dominant sequence *approach, stare, pace* occurred 46 times in combination with various submissive responses and was by far the most common three-category combination. Simply adding *lipsmack* (*approach, stare, pace, lipsmack*) to the dominant side accounted for 35 more chains, but now the submissive animal saw this grouping as much less dominant—out of proportion to the scale value of *lipsmack*. Eighteen responses (50%) to this half-chain were of the friendly type. The addition of one more submissive category to this sequence made a very friendly sequence which was particularly used by males to approach an estrous female. The sequence *approach, stare, pace, lipsmack, walk away* was seen 20 times, while the sequence *approach, stare, pace, lipsmack, present* was seen 21 times, all occurring in male–female pairings. Eighty-five percent of responses by females to these two sequences were of the friendly type, though the summed scale value of these half-chain sequences was still .90 to 1.10 on the dominance–submission dimension. Observations such as these, showing the effects of adding one more category to a sequence, coupled with the above mentioned ambiguity in the meaning of singly emitted categories such as *mount, approach,* and *stare* whose scale values fall between 1.00 and 2.00, support the idea that the monkey must produce a complex series of behavior categories, at least initially, to communicate with any precision in this range of the dominance–submission dimension. Only after a relationship is well established and ongoing in a mutally understood context do two-step chains acquire the necessary communicative precision.

Not all within-group populations could be considered completely homogeneous in this study because some female pairings involved estrous females, and relationships in these pairings were different than in nonestrous ones. In 20 of 60 female–female pair sessions one female was in estrus, and in two more sessions both pair mates were. In the 60 female–male pairings where the females were index animals, the dominant- and low-rank females were in estrus in seven pairings and the middle-rank female in estrus in five pairings. In the 60 male–female pairings where males were the index animals, four females were in estrus when paired with the dominant male and nine were in estrus when paired with both the middle- and low-rank males. Thus, approximately one third of all male–female pairings (41 of 120) involved estrous females. These two subgroups, nonestrous female–male and estrous female–male pairs, provided behavioral data for study of the effect of estrus on positive social bonds. Carpenter (1964) has reported that the estrous female keeps approaching males until they respond adequately, that such approaches are met with initial resistance to close association, and that the male may repeatedly attack her. He noted that nonestrous females withdraw when the male approaches, whereas the estrous female advances. Data from the present study provided an opportunity to quantitatively corroborate this analysis. All instances were recorded when the female was attacked or threatened, when she approached the male, and when mount–present sequences occurred. The length of all grooming episodes was measured. Contrary to Carpenter's report, the mean number of occurrences of attacks and threats by males on females per half-hour session was slightly lower for estrous than for nonestrous female-paired sessions respectively (.50 vs. .75 and 3.40. vs. 5.00). However, the mean number of approaches to a male in each session by estrous females was increased (12.70 vs. 6.50), the mean time of mutual grooming episodes was three times as long in estrous female–male pairings (10 min vs. 3 min), and the mean number of mount–present sequences per session was twice as frequent (6.00 vs. 3.00). These differences indicate the important role that estrus plays in promoting positive male–female social relationships.

Several studies report frequent observations of monkeys sniffing objects and other monkeys in various contexts, but no specific conclusions have been drawn regarding their importance (Altmann, 1962; Hinde & Rowell, 1962). In the present study, when males and females unfamiliar to each other were introduced in close proximity sniffing was a very obvious, immediate, and universal behavior pattern shown by the male. When the male first approached, females invariably presented and the male advanced, sniffing and closely inspecting the female's perineum. If the female was in estrus the male immediately demonstrated friendly chain sequences, whereas if she was not he walked away and ignored her further behavior. Michael and his colleagues (Michael & Keverne, 1968; Michael, Keverne, & Bonsall, 1972) demonstrated that anosmic rhesus males do not respond sexually to females receiving intravaginal estrogen, and that short-chain aliphatic acids in the vaginal secretions of estrous females are responsible for stimulating sexual behavior in males. From the differences noted above in the approach behavior, grooming, and frequency of friendly behavior chains for males paired with estrous females, it seems clear that pheromones are responsible for changes in the relationship which are much more generalized than simple male sexual arousal.

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Residual Fear of the Conditioned Stimulus as a Function of Response Prevention after Avoidance or Classical Defensive Conditioning in the Rat

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SUMMARY

Studies employing response prevention (RP) are reviewed. Considering assessment difficulties and conflicting findings, it is questionable whether RP actually reduces fear to a conditioned stimulus (CS). This study measured fear after RP via a conditioned emotional response (CER) paradigm. Hypotheses were that fear of an auditory CS (conditioned in an avoidance paradigm) is reduced during RP, and that fear conditioning would occur to aspects of the conditioning environment per se. Also evaluated was the effectiveness of RP when fear had been learned under two different conditions: (a) avoidance or (b) classical defensive conditioning.

Seven groups of 10 experimentally naive female rats were run. Animals were initially trained to bar press for food pellets on a variable interval (VI) 2 schedule. Three groups were then avoidance trained in a two-way shuttle box to a criterion of 10 successive avoidances. Immediately following acquisition, one group received RP (blocking) in the shuttle box (Condition A-B). This consisted of placing a door between the two sides of the box and presenting the 85 dB (A) white noise CS for 15 20-sec periods with a variable 1-min interstimulus interval. One group did not receive RP (nonblocked) and was instead immediately returned to its home cage (Condition A-NBHC). The third group was treated as was A-B except the CS was not presented (Condition A-NBSB). Two other groups were trained in a classical defensive paradigm. These animals were matched to A-B animals in terms of number, order, and duration of CSs and USs. Following conditioning, one group received the same treatment as A-B (Condition CD-B), and the other received the same treatment as A-NBHC (Condition CD-NBHC). Two groups served as controls. A backward control (Condition BC-NBHC) was matched to A-NBHC in terms of number, order, and duration of CSs and USs. A sensitization control (Condition SC-NBHC) was matched to A-NBHC in terms of number, order, and duration of CS presentations. Immediately following conditioning trials, control animals received the same treatment as A-NBHC animals. After differential treatments all animals were immediately returned to the lever box in which they had learned to bar press, a VI 2 schedule was reinstated, and the CER was measured.

A-B showed significant suppression initially but significantly less than A-NBHC, suggesting that although RP was effective in reducing fear to the CS, some fear remained. Controls showed essentially no suppression and did not differ. A-B did not differ from A-NBSB, suggesting that conditioning of fear did occur to the environment and that this fear was subsequently reduced in A-NBSB. A-B suppressed significantly more than CD-B, suggesting that RP was more effective when fear was learned in a classical as compared to an avoidance paradigm. Theoretical implications and generalizations to implosive therapy are discussed.

Learning models have been frequently employed to describe the development of behavior pathology and of behavior change as a function of some form of treatment. Recently there has been a growing interest in examining the appropriateness of the analogy between certain forms of neurotic behavior and avoidance learning. Two-process fear mediation theory has most often provided the framework. The development and maintenance of neurotic symptoms have been described in terms of classical conditioning of fear to stimuli and the subsequent acquisition of instrumental responses that reduce contact with these stimuli and are reinforced through fear reduction (e.g., Mowrer, 1960; Rescorla & Solomon, 1967). There are generally two components of the avoidance response thought to be changeable. The first is a classically conditioned fear response (CR) to the conditioned stimulus (CS) which is learned as a result of the pairing of the CS with an aversive unconditioned stimulus (US). The fear response and its afferent feedback are typically assigned motivating and rewarding properties. The increase in the sensory feedback from the fear response is postulated to instigate the instrumental component of the avoidance response. Reduction in the sensory feedback from the fear response is postulated to reinforce the instrumental component. Thus, the avoidance response relieves the organism of the noxious quality of the feedback from the CS. Writers such as Solomon, Kamin, and Wynne (1953) suggest that because the instrumental avoidance response is effective in reducing the noxious classically conditioned fear, it is maintained at a high rate when the CS is presented.

More recent theories of avoidance learning have emphasized discriminative stimulus (Herrnstein, 1969) or cognitive–expectancy (Seligman & Johnston, 1973) explanations. Herrnstein's (1969) position asserts that external signals in the avoidance situation become discriminative stimuli which "set the occasion" for the avoidance response. These external signals inform the animal that it is now in that part of the session during which responding produces reinforcement. D'Amato (1970) suggests that the only other role for external signals is that their response-contingent offset may help the subject to "discriminate the avoidance contingency." Reinforcement for the response is provided not by the removal of the CS but rather by the absence of shock itself. Seligman and Johnston's (1973) cognitive–expectancy account of avoidance learning includes two components. The cognitive component assumes that an animal has a preference for no shock over shock, and acquires two expectancies: (a) that responding leads to no shock, and (b) that not responding leads to shock. The presence of the stated preference and pair of expectancies is a sufficient condition for responding to occur. The second or emotional component is based on the elicitation of responses by classically conditioned fear.

The conditioned avoidance response (CAR) is considered "neurotic" when it occurs in the absence of serving any adaptive function (e.g., allowing the organism to avoid an actual aversive stimulus situation). The development and maintenance of "neurotic symptoms" have typically been described in terms of classical conditioning of fear to stimuli and the subsequent acquisition of a response that avoids or escapes contact with these stimuli. Some researchers (e.g., Stampfl & Levis, 1967) have addressed some of the implications of this two-process model for the elimination of maladaptive behavior. However, other implications seem to have been neglected (Riccio & Silvestri, 1973). The problem for the clinical practitioner as

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well as the experimentalist has been to derive effective means of eliminating or at least reducing maladaptive avoidance behavior.

Solomon, Kamin, and Wynne (1953) were perhaps the first to derive an effective method of reducing nonfunctional avoidance behavior. After using ordinary extinction procedures that proved unsuccessful, these experimenters introduced a response prevention (blocking) procedure, which consisted of physically preventing the avoidance response in the presence of the feared CS. This procedure has served as the animal analogue model of the behavior therapy technique known as implosion.

Although numerous studies (e.g., Bankart & Elliott, 1974; Baum, 1969a, 1969b, 1970; Berman & Katzev, 1972; Schiff, Smith, & Prochaska, 1972; Shipley, Mock, & Levis, 1971; Solomon et al., 1953) have demonstrated that the response prevention procedure has been useful in reducing the resistance to extinction of the CAR, the mechanisms underlying its effectiveness are not clearly understood. This is reflected in the fact that several discrepancies are reported in the recent literature. For example, experiments designed to study the most effective use of response prevention time in reducing the CAR are somewhat at odds. Schiff, Smith, and Prochaska (1972) exposed 15 groups of rats to either 1, 5, or 12 blocking trials lasting for either 0, 5, 10, 50, or 120 sec following avoidance training of a 175-V US. The animals were trained in an alley apparatus to a criterion of 10 consecutive avoidances. To determine if there was a differential effect between the number of blockings and length of blocking, three sets of groups were compared. One set (10 blockings of 5 sec vs. 1 block of 50 sec) had a total blocking time of 50 sec: a second set had a total blocking time of 120 sec (12 blockings of 10 sec vs. 1 blocking of 120 sec); and the third set had a total blocking time of 600 sec (5 blockings of 120 sec vs. 12 blockings of 50 sec). No differences within any of the three sets were found. These results indicated that total response prevention time, as opposed to either the number of blocked trials or the length of each blocked trial, was the critical variable for effective blocking. In another experiment, however, Berman and Katzev (1972) report that distributed blocking (40 CS exposures) was more effective than a single massed blocking trial of equal total time when rats were run in a shuttle box with a 2.0-mA US. It should be pointed out that the data of Berman and Katzev are easier to explain in the context of Rescorla and Solomon's (1967) contingency theory, which holds that extinction of conditioned fear is a function of CS–US contingency reduction. Since each nonreinforced presentation of the CS reduces the CS–US contingency, this theory would appear to predict that extinction of conditioned fear is largely a function of the number of nonreinforced CS exposures with exposure duration having little effect.

In order to clarify the discrepancy between the Schiff et al. (1972) and the Berman and Katzev (1972) studies, procedural differences between them deserve comment. First, Schiff et al. used a maximum treatment time of 10 min, while Berman and Katzev's groups remained in treatment for 27 min. Secondly, different apparatuses were used in the two studies. Schiff et al. used an alley, while Berman and Katzev used a shuttle box. It is possible that during blocking in the shuttle box discrete CS presentations may have a much greater effect on subsequent behavior than a prolonged exposure of comparable duration. This may be due to the fact that in this apparatus, during conditioning, the animal is required to escape the CS by running into a compartment where it was recently shocked. In the alley situation, the rat always avoids or escapes by running away from the same box, that is, away from the CS area. Pursuing this argument, we may speculate that CS onset in the Berman and Katzev study took on a greater discriminative role since it was the only consistent CS. That is, it alone was always paired with the US—other environmental cues (e.g., positional cues) were irrelevant. On the other hand, the white noise CS in the Schiff et al. study was not the only stimulus that was consistently present upon US presentation. Environmental-positional stimuli were also consistently paired with the US.

If the CS onset in the Berman and Katzev study served a greater discriminative role than CS onset in the study by Schiff et al., then it would be reasonable that more CS onsets during response prevention would be necessary in order to facilitate extinction as compared to one long CS presentation. Conversely, since CS onset played a lesser role in conditioning in the Schiff et al. study, differences in response prevention treatments might not be expected to produce significant differences. Several researchers (e.g., McAllister & McAllister, 1971) have pointed out the important role of fear conditioned to static apparatus cues in addition to that conditioned to the discrete CS when interpreting results of any study involving fear conditioning. Indeed, Shipley et al. (1971) have also pointed out the important influence of extinction of shock compartment apparatus cues in extinction of the CAR.

Another line of argument relevant to the discrepancy concerning the most effective use of extinction time has been succinctly outlined by Shipley (1974). He points out that in order to investigate the relative contribution of the number and duration of CS exposures, not only should total CS exposure be held constant while the number and duration of CS presentations are allowed to vary inversely, but the amount of fear conditioned to the CS should be held constant. This is clearly not the case in the avoidance paradigm, since the animal's behavior determines the duration of the CS and US and the number of CS–US pairings. If extinction of avoidance responses is used as the index of fear response strength, then an additional problem is introduced in that the duration of nonreinforced CS exposure in the extinction phase is also partially determined by the subject. Shipley's (1974) study employed Pavlovian fear acquisition and extinction procedures in a factorial design in which CS duration in acquisition, the number and duration of CS exposures in extinction, and total CS exposures across extinction trials were varied. Results of this study, which employed suppression of licking for water as the measure of residual fear, revealed that suppression was an inverse function of total nonreinforced CS exposure irrespective of the number and duration of extinction exposures used to amass that total. Earlier data reported by Shipley et al. (1971) also suggested that fear reduction is a function of total CS exposure irrespective of number of trials. Thus, Shipley's findings support those of Schiff et al. (1972) and are not supportive of Rescorla and Solomon's (1967) contingency theory of Pavlovian conditioning.

Discrepant findings have also resulted from studies conducted to determine the appropriate amount of response prevention time that is necessary for effective facilitation of extinction. Baum (1969a, 1969b) has studied this problem in a series of experiments in which he employed a box apparatus with an automated sliding platform located 6 in. (15.24 cm) above a grid floor. When the platform was inserted into the box, the rat could escape the CS and avoid the US by jumping on it. Subsequently, response prevention could be initiated by withdrawing the platform, thus making it unavailable to the rat for an avoidance response. Baum (1969a) reported that increasing the duration of response prevention increased its effectiveness in hastening extinction. Three to 5 min of response prevention was the minimum effective time for rats after receiving a mild (0.5-mA) US. Baum (1969a) also parametrically varied the intensity of shock used. Results showed that intense shock (1.3 or 2.0 mA) diminished the efficacy of a fixed amount of response prevention (3 min). Siegeltuch and Baum (1971) report results somewhat different from those of Baum (1969a). These experimenters administered 40 distributed 1-sec .5-mA unavoidable shocks (10 shocks on each of the 4 days preceding avoidance training) prior to avoidance training to ensure that the fear mediating the avoidance response was "deeply rooted." Results revealed that 30 min of response prevention was necessary to significantly reduce the CAR. Also, 30 min of response prevention was significantly more effective in reducing the CAR than either 0, 5, or 15 min of response prevention. As Baum (1969a) points out, several factors—including amount of response prevention given, US intensity, and whether or not the response has been overtrained—influence the efficacy of response prevention in facilitating the extinction of an avoidance response.

A problem closely allied with that of determining optimal response prevention time is the problem of understanding how an animal's behavior during response prevention interacts with this treatment. Bearing on this question, Baum (1969a) suggested that the effectiveness of response prevention is related to the behavior of the animal during response prevention. Baum and Gordon (1970), employing a box apparatus, found a significant relationship between fearful behavior during response prevention and the subsequent extinction test. These experimenters measured fearful behavior during blocking by employing a time-sampling technique in which the rat was observed, and the main activity of the rat was recorded. Findings suggested that the more fear behavior displayed during response prevention, the more the animal will continue to respond in the extinction test that follows. Spring, Prochaska, and Smith (1974) found exploratory behavior to be significantly related to fear reduction when measured by approach behavior into the formerly feared CS area of an alley apparatus. These studies are discrepant with some recent data (Spring, 1973) which suggest that exploratory behavior is not related to approach-measured fear reduction. In these later studies Spring employed a box and platform apparatus. Although there are several procedural differences that may have contributed to these discrepant findings (e.g., alley vs. box apparatus), a more salient problem in evaluating Spring et al.'s (1974) findings is that the use of approach into the CS area as the dependent measure of fear made for a somewhat circular argument in that those animals that showed fear reduction (by actively approaching the CS) were those of a subgroup (of the original experimental group) selected because they initially showed a great deal of exploratory behavior during blocking.

Shipley et al. (1971), after a series of two one-way shuttle box experiments that carefully measured activity level of the rat via an ultrasonic motion detector, noted that although response-prevented subjects were generally inactive during the prevention phase and subsequently extinguished rapidly, it still remained unclear whether low resistance to extinction in response-prevented subjects was actually mediated by low-activity level. The results of a third experiment reported in the Shipley et al. (1971) article suggested "that fear reduction occurs independently of activity level during CS exposure and of the presence or absence of active CARs" (p. 269). The authors concluded that although their results do not support the hypothesis that extinction after response prevention is solely a result of incompatible response tendencies, their data do not rule out the possibility that an interaction between fear extinction and incompatible response tendencies could operate to produce fast extinction. Thus, results from these experiments suggest that there may not be a direct relationship between the elimination of a CAR and the extinction of fear-motivating properties of the CS.

Results sometimes viewed as discrepant with those that suggest that response prevention decreases resistance to extinction are those provided by recent studies suggesting the possibility of a paradoxical enhancement of fear. Studies done by Coulter, Riccio, and Page (1969) and Rohrbaugh, Riccio, and Arthur (1972) have shown that with relatively short CS presentations during response prevention there seems to be a paradoxical enhancement of fear to the CS. Linton, Riccio, Rohrbaugh, and Page (1970) reasoned that if response prevention trials after avoidance training enhanced fear, then they might find greater residual fear in blocked animals than in animals that were simply tested after reaching an avoidance criterion. Linton et al. tested this notion in an experiment that employed a one-way shuttle box and a combination of extinction and/or blocking treatments. Results showed that although response prevention reduced the resistance to extinction of the CAR, a subsequent test indicated that passive avoidance of the former shock chamber was greater in blocked and extinguished as compared to nonblocked and extinguished animals. More recently, Marrazo, Riccio, and Riley (1974) have reported data suggesting that response prevention with shock increased latency-to-eat-measured fear, but facilitated avoidance extinction.

While considerable caution must be exercised in extrapolating experimental data to the clinical setting, it may be instructive to point out that discrepancies among treatment results have also been frequently found with human research. Reported results from implosive therapy range from fear reduction to fear enhancement (Ayer, 1972; Hodgson & Rachman, 1970). Noting obvious problems, critics have challenged the appropriateness of using response prevention procedures with humans in therapy situations. Most recent among such critics, Morganstern (1973, 1974) has charged advocates of implosive therapy with a lack of experimental rigor. Indeed, Eysenck (1968) has called for the postponement of response prevention procedures until more detailed and reliable guides have been provided by analogous studies.

A review of both the experimental animal studies and the clinical studies clearly suggests that although response prevention techniques are effective in some circumstances, they are not effective in others. Inherent in studying this obvious discrepancy of results is the problem of what to use as an appropriate measure of fear. Typically, either one of two dependent fear reduction measures has been employed. The first simply involves measuring the resistance to extinction of the CAR after avoidance conditioning and response prevention. Here, the assumption usually made is that more rapid extinction of the CAR in the blocked, compared to the nonblocked, groups implies that the fear response has been reduced by the response prevention procedure. Unfortunately, investigators employing this measure are limited in their interpretations of fear reduction, since the procedure has obvious methodological weaknesses. Several experimenters (e.g., Page, 1955; Page & Hall, 1953) have suggested that reduction in this dependent fear measure may actually be a result of learning a new response to the still feared CS rather than a reduction of fear. For example, instrumental responses other than avoidance, such as freezing, crouching, or grooming, may be learned during response prevention. These alternative responses may persist during fear testing such that the probability of occurrence of the previously trained CARs is decreased due to the presence of an incompatible but unmeasured alternative avoidance response.

A second dependent fear measure frequently employed has been approach behavior into the CS area (Nelson, 1969; Page, 1955; Spring et al., 1974). The rationale here has been that if the organism fears the CS then it should not approach it. As Spring et al. (1974) point out, approach measures eliminate the aforementioned problems, which are characteristic of CAR extinction measures. Unfortunately, approach measures introduce special problems of their own, since motivation for approaching the CS must be introduced. Although the importance of motivation is not to be underestimated, its interaction with other relevant variables (e.g., length of response prevention) is as yet unknown, and it would seem that the potential for undetected interaction and confounding is great. Also, it should be noted that Spring (1973) manipulated motivation through food deprivation and found that it had no apparent effect. Difficulties inherent with approach behavior as a dependent measure of fear are further complicated by findings that suggest, on the one hand, a correlation between exploratory behavior and fear reduction measured via CAR extinction (e.g., Baum & Gordon, 1970) and, on the other hand, only a partial relationship between exploratory behavior and subsequent approach-measured fear indices (e.g., Spring, 1973).

It is apparent from the preceding review that many studies report a facilitation of extinction as a function of response prevention. However, the process by which response prevention produces this extinction is not altogether clear. This fact is emphasized by the findings (e.g., Rohrbaugh et al., 1972) that suggest a paradoxical enhancement of fear. Another major point in the review has been the importance of considering the technique that is employed in evaluating fear after response prevention. Also, some of the discrepancies in the literature have been discussed with particular emphasis on the differences in apparatuses employed. These considerations have suggested that problems inherent in assessing fear reduction as a function of response prevention are numerous and their implications for any theorizing as to what mechanisms might be operative in response prevention are indeed basic. Since we cannot understand what processes are operating in response prevention unless we have precise control of the antecedent conditions and precise and meaningful measurement of the dependent fear, whether or not response prevention actually reduces fear remains unknown. What is needed is a measure of fear unconfounded by responses present during fear acquisition.

In the present study the conditioned emotional response (CER) paradigm (Estes & Skinner, 1941) was used to measure conditioned fear. The CER consists of the suppression of ongoing instrumental behavior in the presence of a warning signal (CS) that has preceded shock (US). It is generally considered that the CER procedure has provided more complete and reliable information than any other method regarding the determinants of conditioned fear (Church, 1971). The effects of fear to the CS were measured independent of avoidance behavior in an environment different from that in which fear was initially conditioned. The CER measure was chosen not only because of its reliability and precision but also for its appropriateness to the clinical analogue. If maladaptive avoidance behaviors interfere with an individual's day-to-day activities, then this may be analogous to the CER situation in which a suppression of ongoing behavior is observed upon the presentation of a CS.

The first hypothesis was that fear of an auditory CS is reduced during response prevention. It was predicted that blocked animals would show significantly less response suppression in a subsequent CER test than nonblocked animals. Another hypothesis was that fear conditioning occurs to the conditioning environment per se. Evidence from pilot studies conducted in our laboratory suggested that animals which were supposedly nonblocked in the conditioning apparatus (shuttle box) showed little difference in subsequent CER tests when compared to blocked animals. Thus, it seemed that the conditioning environment was acquiring special CS properties. To evaluate the effect of response prevention without CS presentation, two nonblocked groups were included. One was nonblocked in a compartment of the shuttle box, the other was nonblocked in the animal's home cage. The prediction was that animals nonblocked in the shuttle box would show less suppression than animals nonblocked in their home cages. Another interest was in evaluating the effectiveness of response prevention when fear was learned under two different conditions. One condition, avoidance, provided the animal with the opportunity to learn to actively avoid the US. The second, classical defensive, did not afford the animal the opportunity to avoid. No differences were expected between these groups if two-process theory is correct, that is, if fear is learned merely as a function of CS–US pairings. In this case, the presence of an active response should not influence the amount of fear conditioned to the CS.

METHOD

Subjects

Subjects were 70 experimentally naive female albino rats of the Sprague-Dawley strain. When run in the study, their ages ranged from 75 to 130 days and their weights from 172 to 240 g. They were housed individually and were fed ad lib Wayne Lab-Blox brand food pellets.

Apparatus

The avoidance apparatus was a modified Miller-Mowrer shuttle box (51 × 16.5 × 25.5 cm). It had metal sides, a clear Plexiglas top and brass grids spaced 1.25 cm apart at the floor. The box was divided by a metal wall that had an 11.5 × 9 cm opening through which the rat could pass. A black door fitted into the opening such that when present, the rat could not shuttle from one side to the other. The ambient noise level of the shuttle box was 64 dB (A). The CS was an 85-dB white noise delivered through speakers. The US was a matched-impedance scrambled shock of 225 V, ac with 150-kΩ resistance in series with the rat.

TABLE 1

DESIGN OF THE STUDY

Treatment	Avoidance conditioning (A)	Classical defensive conditioning (CD)	Backward control (BC)	Sensitization control (SC)
Blocked (B)	A-B	CD-B[a]		
Nonblocked in home cage (NBHC)	A-NBHC	CD-NBHC[a]	BC-NBHC[b]	SC-NBHC[c]
Nonblocked in shuttle box (NBSB)	A-NBSB			

Note. N of each cell = 10.

[a] Subjects matched to A-B in terms of number, duration, sequence of CS, US exposures, as well as number of CS exposures beyond those paired with USs.

[b] Subjects matched to A-NBHC in terms of number, duration, sequence of CS, US exposures, as well as number of CS exposures beyond those paired with USs.

[c] Subjects matched to A-NBHC in terms of number, duration, and sequence of CS exposures.

Two typical Plexiglas and aluminum lever boxes, 24 × 20.50 × 19.75 cm, were located in a second room. The grids on their floors were spaced 1.25 cm apart. One end wall contained a lever and a food cup. Each lever box was in its own sound-deadening chamber. The CS was delivered to each lever box through a speaker mounted on its ceiling. The ambient noise and CS levels were the same as those in the shuttle box.

Procedure

Animals were reduced to 75%–80% of their preexperimental body weights 1 week prior to participating in the experiment and were maintained there. Water was continuously available in the home cages. All animals were treated identically during Phases 1 and 3.

Phase 1: Magazine and Bar-Press Training

During Session 1 all animals were placed in a lever box for 3 hr. During the first hour Noyes pellets were available on a continuous reinforcement (CRF) schedule. Also, free pellets were provided at 5-min intervals. Animals were gradually shaped to a variable interval (VI) 2 schedule. During five additional 2-hr. sessions animals pressed on a VI 2 schedule. For inclusion in the study animals had to meet a criterion of at least 1,500 responses per session for Sessions 3 to 6.

Phase 2: Conditioning and Blocking

Each animal was randomly assigned to one of seven groups. Approximately 24 hr. after the sixth session, animals were individually run in the shuttle box. Animals were given a 5-min adaptation period in the shuttle box during which the CS tone was not present. In order to assure that subjects received at least three shocks, all animals, except those in the sensitization control condition, initially received either three unavoidable CS–US pairings or three unavoidable US–CS presentations depending on their experimental condition.

Avoidance training and blocking. Three of the seven groups were trained to avoid the US to a criterion of 10 successive avoidances. The CS–US interval was 20 sec. If the animal moved to the opposite side of the apparatus during this 20-sec interval, the CS was terminated. If the animal failed to avoid during the CS–US interval, then the US came on until an escape response was made at which time both the CS and US terminated. Intertrial intervals (ITI) were arranged according to a predetermined variable 1-min schedule. Table 1 summarizes the conditions used in the study. Upon reaching the 10 consecutive avoidances criterion, avoidance-trained animals (A) were immediately given one of three treatments for a period of 20 min: (a) blocked (A-B), (b) nonblocked in home cage (A-NBHC), or (c) nonblocked in shuttle box (A-NBSB).

Animals in the A-B group remained on the side of the box to which they last ran. The door was placed between the two sides of the box, thus preventing further crossings. The CS was then presented for 15 20-sec periods with a variable 1-min interstimulus interval (ISI). Animals in Group A-NBSB were treated identically to animals in Group A-B, except that the CS was not presented. Animals in Group A-NBHC, upon reaching the avoidance criterion, were removed from the apparatus and placed in their home cages for 20 min, the amount of time needed for the A-B and the A-NBSB procedures.

Classical defensive training and blocking. Two groups were trained in a classical defensive paradigm. The CS–US interval was 20 sec. One classical defensive group (CD-B) was blocked in the shuttle box. This consisted of placing a door between the two sides of the box and presenting the CS for 15 20-sec periods with a variable 1-min ISI. This was a classical conditioning extinction procedure. Animals in the other classical defensive group (CD-NBHC) were nonblocked in their home cages for 20 min, and thus not exposed to an extinction procedure. Animals in both classical defensive groups were matched as indicated in Table 1.

Control groups. Two control groups were run: (a) a backward conditioning control (BC-NBHC) controlling for pseudoconditioning and sensitization and (b) a sensitization control (SC-NBHC).

Animals in both groups were nonblocked in their home cages. Animals in Group BC-NBHC always received the US from 5 sec to 10 sec prior to CS onset. The ISI was determined randomly. Animals in this group were matched to animals in Group A-NBHC as indicated in Table 1. Animals in Group SC-NBHC received CS presentations only. These animals were also matched as indicated in Table 1.

Phase 3: CER Test for Fear

Immediately following Phase 2 animals were placed in the lever box and the VI 2 schedule was reinstated. After 4 min, a series of 12 20-sec periods were presented with a 4-min ITI. Bar presses during 20-sec CS and pre-CS periods were recorded. The US was never presented in the lever box. Fear was measured by a "suppression ratio" ($B/A+B$, with B representing presses during the CS and A representing presses during the 20-sec pre-CS periods). Thus, a ratio of .50 represented no effect of the CS, a ratio of 0 represented complete suppression, and a ratio greater than .50 represented increased pressing during the CS compared to the preceding 20 sec. This particular measure of suppression was selected because it is well established by precedent in the literature that measures fear after avoidance conditioning (e.g., Kamin, Brimer, & Black, 1963).

RESULTS

Avoidance Training

Results of analysis of variance performed on avoidance groups to test for possible differences in avoidance learning indicated no differences for (a) number of trials to criterion, (b) number of avoidances, and (c) time exposed to US. This indicated that no differences existed among these groups after avoidance training.

Responding During First Pre-CS Period of CER Phase

Results of analyses of variance done on the first 20-sec pre-CS periods to assess overall response rate of each group independent of the effect of CS periods showed a nonsignificant F. This indicated that the experimental condition did not differentially influence overall rate of pre-CS responding.

CER Test for Fear: Avoidance Conditions

Suppression data for the avoidance-trained conditions are illustrated in Figure 1. To assess suppression ratio differences across Trials 1–12 for these conditions, a two-way repeated measures analysis of variance was performed on the mean response suppression ratio for blocks of three trials. Results of this analysis showed a significant conditions effect, $F(2, 87) = 6.20$, $p < .01$, a significant trials effect, $F(3, 261) = 15.71$, $p < .01$, as well as a significant conditions by trials in-

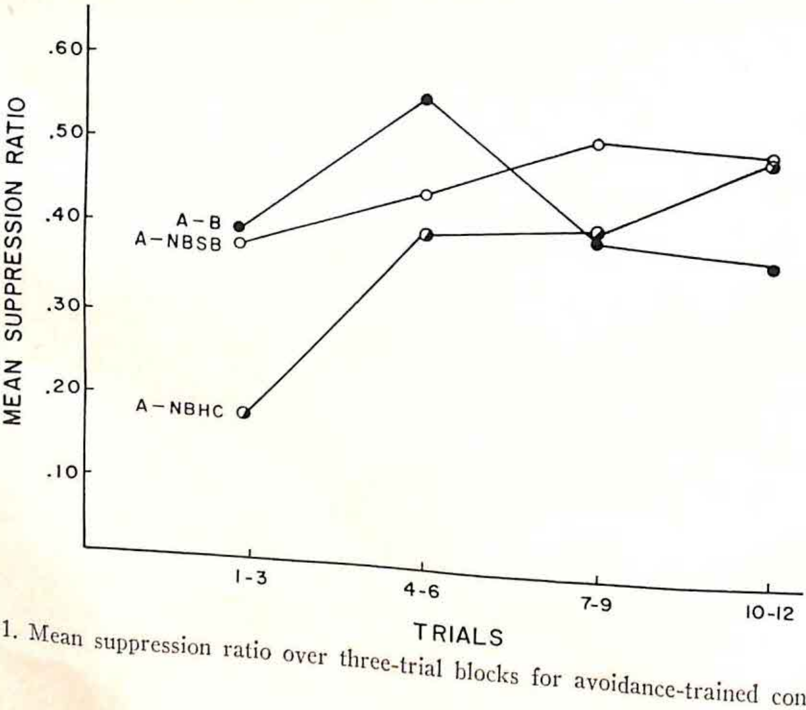

FIGURE 1. Mean suppression ratio over three-trial blocks for avoidance-trained conditions.

teraction, $F(6, 261) = 7.70$, $p < .01$. Since both trials effect and the interaction are apparent from Figure 1, multiple comparisons were done only on the conditions effect. According to Dunn's test, Condition A-NBHC was significantly different from Conditions A-NBSB and A-B, $p < .01$ and $p < .05$, respectively. Thus, most response suppression was found in A-NBHC. A-B and A-NBSB showed significantly less response suppression, and they did not significantly differ from each other.

To more closely assess differences in suppression for avoidance-trained conditions, mean suppression ratios for Trials 1–3 are illustrated in Figure 2. It is apparent that A-NBHC shows more suppression than A-B and A-NBSB. To test this difference, a two-way repeated measures analysis of variance was done. Results of this analysis showed a significant conditions effect, $F(2, 27) = 9.92$, $p < .01$, as well as a significant trials effect, $F(2, 54) = 15.56$, $p < .01$. According to a Dunn's test, A-NBHC significantly differed from A-B and from A-NBSB, both $p < .01$. The significant trials effect is apparent in Figure 2. It is also apparent that A-B and A-NBSB were not significantly different from each other.

Suppression differences between avoidance-trained conditions on Trial 1, were also analyzed using Dunn's procedure. The results showed that A-NBHC significantly differed from both A-B and A-NBSB, $p < .05$.

CER Test for Fear: Classical Defensive Conditions

Mean response suppression ratios for Conditions CD-B and CD-NBHC are illustrated in Figure 3. It is apparent that only Trials 1–3 show considerable suppression. Figure 3 also shows considerably more suppression for CD-NBHC as compared to CD-B on Trials 1–3. Mean suppression ratios for these trials are illustrated in Figure 4. Also illustrated in Figure 4 are the mean suppression ratios for A-B, to which CD-B and CD-NBHC had been matched. Figure 4 suggests that most of the suppression differences between CD-B and CD-NBHC were due to the difference on Trial 1 where CD-NBHC shows far more suppression than does CD-B.

In order to assess differences between CD-B and CD-NBHC as well as to compare these conditions to A-B, a two-way repeated measures analysis of variance was done comparing mean suppression ratios of these three conditions. Results showed only

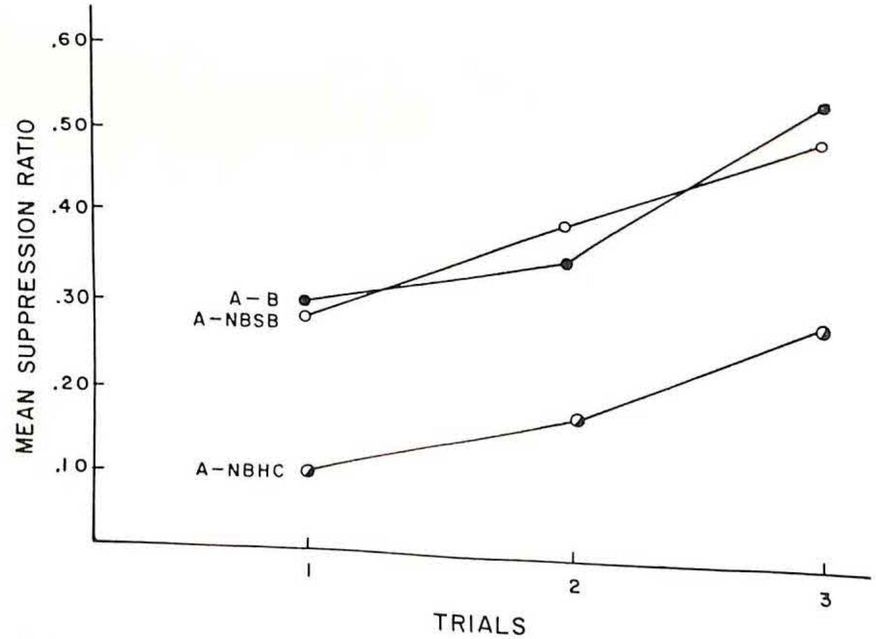

FIGURE 2. Mean suppression ratio over Trials 1, 2, and 3, for avoidance-trained conditions.

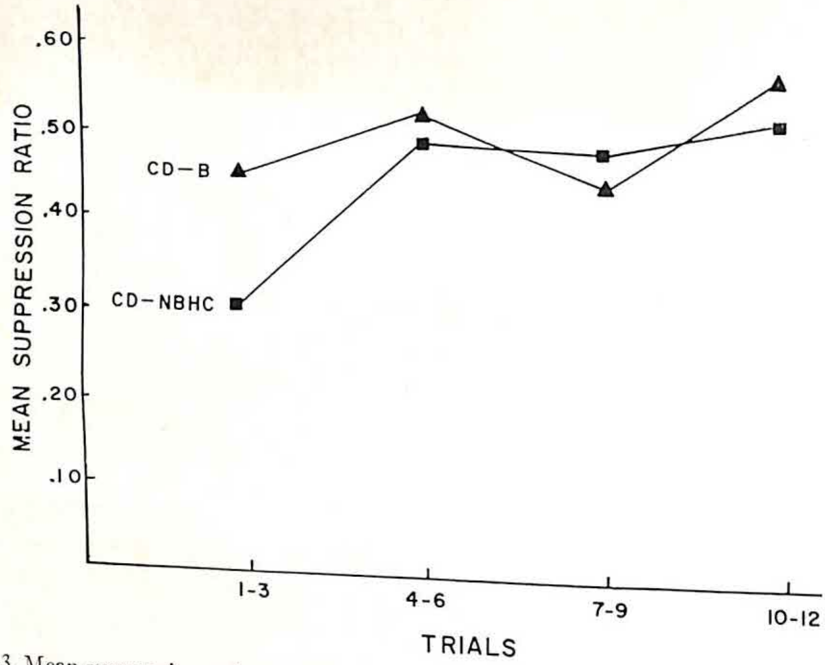

FIGURE 3. Mean suppression ratio over three-trial blocks for classical-defensive-trained conditions.

a significant trials effect, $F(2, 54) = 11.35$, $p < .01$. This trials effect represents a typical extinction of the suppression as a function of nonavoidable CS presentations during testing. Dunn's procedure was used to assess differences between CD-B, CD-NBHC, and A-B on Trial 1. Results showed that the only significant difference was between CD-B and CD-NBHC, $p < .01$, suggesting that response prevention significantly reduced suppression when fear was learned in a classical defensive paradigm.

CER Test for Fear: Control Conditions

Mean response suppression ratios for Conditions BC-NBHC and SC-NBHC showed essentially no response suppression and very similar response patterns. These results suggest that there was no evidence for pseudoconditioning or sensitization. To assess differences between these control conditions and Condition A-NBHC to which both controls had been matched, a two-way repeated measures analysis of variance was done on the mean response suppression ratios for these conditions on Trials 1, 2, and 3. The results showed a highly significant conditions effect, $F(2, 27) = 32.65$, $p < .01$. Both trial and interaction effects were nonsignificant. According to Dunn's procedure, A-NBHC significantly differed from both BC-NBHC and SC-NBHC ($p < .01$).

DISCUSSION

The major hypothesis of the study, that fear of a CS as measured in a CER paradigm would be reduced after response prevention, was supported. This result, in general, supports Solomon et al.'s (1953) original theoretical assumption that avoidance reduction after response prevention treatments may be a function of a weakened classically conditioned fear response to the CS. The results also corroborate those of other studies (e.g., Baum, 1969a; Berman & Katzev, 1972; Spring et al., 1974) which suggest that response prevention reduces fear to the CS. However, a distinction should be made between studies reporting merely a reduction in avoidance behavior or an increase in approach behavior in the training environment as a result of response prevention and the present study, which reports a reduction in measured effects of fear to the CS in a different environment. That these two types of results do not necessarily parallel each other has been pointed out by Kamin et al. (1963) and Linton et al.

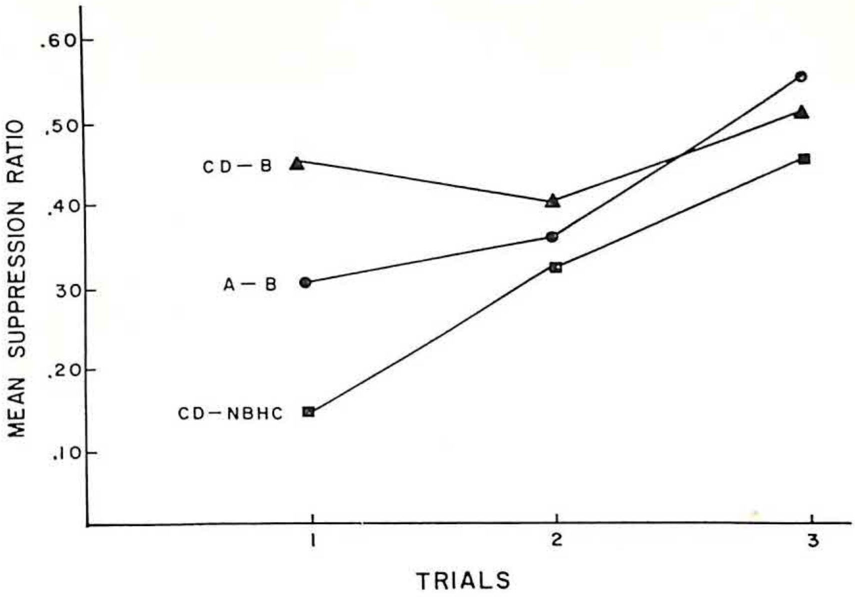

FIGURE 4. Mean suppression ratio on Trials 1, 2, and 3, for classical-defensive-trained conditions and Condition A-B.

(1970), who demonstrated that even after avoidance behavior had been extinguished, there was still much fear to the CS as measured by either a CER technique or passive avoidance of the former shock chamber, respectively.

It should also be pointed out that response prevention in the present study did not completely eliminate the fear response. Indeed, there was significant suppression on Trial 1 for Condition A-B when compared to Control Conditions BC-NBHC and SC-NBHC, $t = 2.24$, $p < .05$. This suggests that blocked animals were still somewhat fearful of the CS, although they were significantly less fearful than animals nonblocked in their home cages (Group A-NBHC). This finding suggests a possible explanation linking those studies which report that response prevention does not lead to a reduction of fear (e.g., Coulter et al., 1969; Page & Hall, 1953), and those studies that suggest that it does (e.g., Baum, 1969a). Perhaps differences in the amount of fear existing after response prevention have been one source of the discrepancy in the response prevention literature. Other measures of fear (e.g., approach into the CS area or extinction of the CAR) might not have detected the differences between complete fear elimination and a partial reduction in fear to the CS, whereas the CER technique did.

The second hypothesis of the study was that the conditioning environment per se would acquire CS properties that would be reduced even though the white noise CS did not come on. This led to the prediction that animals nonblocked in the shuttle box would show less suppression than animals nonblocked in their home cages. The results supported this hypothesis and prediction. In fact, it was found that response suppression in Condition A-B did not significantly differ from response suppression in Condition A-NBSB, suggesting that response prevention in the CS environment was equally effective regardless of whether or not the discriminative CS was presented. This finding is difficult to interpret in terms of classical conditioning principles, since in the shuttle box situation it is assumed that only the white noise CS reliably signals shock. Other stimuli in the environment should have become relatively redundant. This would not be true in an alley or other one-way apparatuses where one might expect spatial cues to be more informative and therefore more effected by response prevention. The present results suggest that the entire conditioning environment acquired aversive properties that were subsequently reduced during Condition A-NBSB even though the white noise CS did not come on. It is possible that the apparatus cues plus white noise could have functioned as a summated CS and that when the apparatus cues were weakened during the A-NBSB condition it caused a reduction in the effectiveness of the white noise component when tested in the lever box. The A-NBHC group would have the white noise and apparatus cues (e.g., grid floor) still effective, because neither of these component CSs would have had their fear CRs extinguished. Thus, the A-NBHC group would show most suppression in the CER test. It would be interesting to know what a classical defensive group would do following exposure to the conditioning apparatus alone without CS presentations. If such a CD-NBSB group responded similarly to the CD-B group, the summated CS explanation would be strengthened.

Also evaluated was the effectiveness of response prevention when fear was learned under two different conditions: avoidance and classical defensive. Before discussing these results some explanation of the matching procedure is in order. Although the yoked control or explicit matching type control procedure has been criticized on several grounds by Church (1964), there are two reasons why it was used in this study. First, the shuttle box used produces very fast learning with relatively little variability. The means of trials to criterion, number of avoidances, and total US time all revealed nonsignificant *F* ratios. The avoidance task was learned by all subjects in under 44 trials, including the 10 criterion trials; there was no indication of a bimodal distribution of learners versus nonlearners; and no subjects were discarded for not learning the avoidance task. Since the subjects were randomly assigned to all groups, and animals from all groups were run in a counterbalanced order, there seems no reason to believe that the animals of the CD groups should be different due to the matching procedure per se. Second, we were especially concerned about assuring that the classical defensive groups received the same number, sequence, and duration of CS and shock exposures. The matching procedure seemed the most feasible way to achieve this goal.

Results of the analysis over Trials 1–3 comparing response suppression for Conditions CD-B, CD-NBHC, and A-B showed a nonsignificant effect of conditions. This is probably due to the fact that although there are some apparent differences on Trial 1, Trials 2 and 3 show similar response patterns (see Figure 4). To further investigate whether avoidance training and classical conditioning interact differently with response prevention as well as to investigate the apparent decrease in the response suppression ratio for A-B on Trials 10–12, which is apparent in Figure 1, additional analyses were conducted. The first was a two-way repeated measures analysis of variance comparing A-B and CD-B with A-NBHC and CD-NBHC on Trial Block 1–3. The results indicated that A-B had significantly more response suppression than CD-B, $F(1, 58) = 4.58$, $p < .05$; and A-NBHC had significantly more response suppression than CD-NBHC, $F(1, 58) = 29.45$, $p < .01$. The interaction was not significant. The second analysis was a two-way repeated measures analysis of variance comparing A-B and CD-B with A-NBHC and CD-NBHC on Trial Block 10–12. The results indicated that A-B had significantly more response suppression than CD-B, $F(1, 58) = 4.71$, $p < .05$. No differences were found between the nonblocked conditions. However, there was a significant interaction between conditioning and blocking treatments, $F(1, 58) = 4.99$, $p < .05$.

These results suggest that there were different amounts of fear conditioned in each paradigm and that response prevention interacted differently with the effects of fear, depending upon the conditioning paradigm. These findings do not generally support a two-factor theory that would predict equal fear conditioning in either the avoidance or classical paradigm. A possible factor contributing to the differences obtained may be the fact that the 10 extinction trials were given before the CER phase (the 10 criterion trials). These extinction trials may have had differential effects on the A-B and CD-B animals, because the A-B animals were not prevented from making their avoidance response until response prevention began.

That the apparent decrease in the response suppression ratio for Condition A-B appears real is somewhat difficult to explain. It may suggest that the fear-reducing effects of response prevention are longer lasting when fear is learned in a classical rather than an avoidance paradigm. A test of the reliability of this finding would seem to be a potentially worthwhile investigation.

Although considerable care must be exercised in extrapolating the present findings to the clinical setting, several generalized implications may be useful. The major contribution of this study was to confirm that response prevention is successful in reducing fear to the CS. However, it is important to point out that response prevention did not completely reduce fear to the CS, in fact, significant fear remained after response prevention. This may be due to the fact that there was not sufficient CS exposure during response prevention. A possible implication is that although implosive therapy may reduce some fear to the CS, significant fear may be present after treatment. Implications for implosive therapy are either that the treatment may not be successful even though some fear reduction has occurred or that the treatment may appear successful, but avoidance behavior may be easily reinstated upon brief presentation of the CS–US contingency at some later time.

A second finding that may be relevant to implosive therapy is that during response prevention the CS environment was equally effective in reducing fear to the CS regardless of the presence of the discriminated CS. This finding may explain why it is not necessary for the therapist to present the original CS in implosive therapy. This suggestion complements Stampfl and Levis's (1967) statement that "complete accuracy is not essential since some effect, through the principle of generalization of extinction, would be expected when an approximation is presented" (p. 499).

Another finding that may be clinically relevant is the fact that response prevention was not equally effective regardless of the original conditions of learning fear. Indeed, response prevention seems to be more effective when fear is learned in a classical conditioning paradigm. This suggests that perhaps whether or not the client was able to avoid the feared object while the original learning of fear was occurring may make a difference in the optimal duration of treatment and overall effectiveness of implosive therapy.

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Comments On: Residual Fear of the Conditioned Stimulus as a Function of Response Prevention After Avoidance or Classical Defensive Conditioning in the Rat

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As Monti and Smith (1976) suggest, the efficacy of response prevention procedures in reducing the resistance to extinction of avoidance responding is an important issue for theories of avoidance learning as well as applied techniques of behavior therapy. The following comments are therefore divided along these lines.

EXPERIMENTAL AVOIDANCE ACQUISITION AND EXTINCTION

Two-factor theories of avoidance behavior (Mowrer, 1947; Rescorla & Solomon, 1967) are generally accepted as the first attempt at explaining acquisition of avoidance responses. They enjoy the status of circumventing teleological explanations of the acquisition of a response that copes adaptively with some event about to occur in the future. According to such theories, Pavlovian contingencies bring about the association of a conditioned stimulus (CS) and noxious unconditioned stimulus (UCS). As traditionally accepted the CS is then capable of eliciting complex conditioned responses (CRs). Among these conditioned reactions are (a) a conditioned fear reaction and (b) some skeletal response that may at first grossly approximate the to-be-learned avoidance response and in time develop into that response.

In 1948 Miller suggested that if Pavlovian conditioning was capable of imbuing a neutral CS with drive properties, then that CS should (a) motivate behavior and (b) when suddenly removed serve as a reinforcement to produce learning of the immediately preceding response. While attempting to explain the acquisition of avoidance behavior, Solomon, Kamin, and Wynne (1953) made this same assumption: They assumed that these emotional reactions (acquired through Pavlovian conditioning during avoidance training) would give rise to stimuli having drive properties. Two-factor theories are a direct extrapolation of this premise. The CS, through conditioning, acquires drive properties capable of instigating avoidance attempts and through its termination becomes capable of reinforcing responses which directly precede that termination. Of course, since the UCS that is employed in the avoidance procedure is typically painful, the acquired drive properties are assumed to be aversive, and the term fear is often substituted as a more specific statement of the prevailing drive state.

It should be clear that such theory should gain empirical support if independent measures of the acquired drive state are shown to vary as predicted with the behavioral events which that drive state is postulated to explain. In the case of avoidance behavior the drive state is fear. Hence, if fear instigates avoidance behavior then correlates of fear should increase prior to the occurrence of those behaviors, and if the avoidance behavior that terminates the CS is supposed to be reinforced by drive (fear) reduction, then correlates of fear should decrease following such behavior and CS termination.

This is the theoretical foundation that formed the basis of the Solomon et al. (1953) researches in avoidance extinction and the Rescorla and Solomon (1967) re-

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view of concomitant measures of autonomic and behavioral indices during instrumental response acquisition. Unfortunately, these studies left the status of two-factor theory in doubt. First, conditions that—according to the two-factor theory—should have led to the extinction of avoidance responding did not; and second, correlations that should have existed between autonomic indices of the motivating drive state and occurrence of the instrumental responses that were instigated and reinforced by that drive state did not exist.

Continued presentation of the CS without the UCS during avoidance extinction should have led—according to two-factor theory—to the eventual cessation of the avoidance response. Absence of the UCS should have extinguished all the acquired drive properties of the CS and thereby remove all instigation to respond to the CS and all reinforcement afforded by CS termination. It did not. Similarly, although *acquisition* of the avoidance response revealed concomitant changes in the level of fear as revealed by autonomic measures, *maintenance* of the avoidance response continued without any indication of the presence of fear.

Solomon et al. (1953) parsimoniously invoked the conservation of anxiety hypothesis to explain both discrepancies. Because the competent avoidance responder never permitted the CS to remain present long enough to produce a respectable fear CR, no concomitance of fear and avoidance responding could be measured and no extinction of the CS–UCS association could take place. This last point is important for two reasons. First, the assumption that fear must be evoked in order to extinguish Pavlovian associations to the CS and in turn remove the motivation and reinforcement for avoidance behavior seems to be a major impetus behind the practice of evoking fear during implosion or flooding therapy. Second, by assuming that protection of the CS from extinction of the fear CR is responsible for the resistance to extinction of the avoidance response, the logical implication which results is that successful extinction of the avoidance response requires the extinction of fear reactions to the CS. Hence, extinguished avoidance responders should not show signs of fear. This directly follows from the conservation of anxiety hypothesis, which itself follows from or is at least necessary for the preservation of the fear mediation two-factor theory.

As it turns out, the blessings that the conservation of anxiety hypothesis bestowed on two-factor theory were more of a hindrance than a help. For example, as Seligman and Johnston (1973) cogently argue in their exposition of a cognitive theory of avoidance learning, two-factor theory requires the elicitation of a fear CR in order to motivate the avoidance response as well as reduction of that CR in order to reinforce it. However, the conservation of anxiety hypothesis assumes that the CS is not present long enough to elicit that CR. Hence, no avoidance response should occur according to this hypothesis. If, on the other hand, the CS is present long enough to evoke small CRs just capable of instigating and reinforcing the avoidance response, then some extinction should take place (see Seligman & Johnston, pp. 81–83 for a detailed account of this argument).

Similarly, as Monti and Smith (1976) point out, several researchers (Baum, 1971; Coulter, Riccio, & Page, 1969; Page, 1955; Page & Hall, 1953) have demonstrated that an avoidance response can be extinguished through response prevention without extinguishing the fear reaction to the CS. If the failure to extinguish the fear reaction to the CS during normal extinction procedures is responsible for the failure of the avoidance response to extinguish, then how can that response cease (as it does with response prevention) while the fear CR remains? Yet, despite this inconsistency with two-factor theory, those experimenters who have noted this contradiction seem to bypass the next logical implication: that fear may not be the critical variable in avoidance behavior. Instead, they appear to assume that fear is indeed responsible for the avoidance behavior, but that response prevention techniques intervene and disrupt the normal flow of events and somehow train in competing responses (which are themselves presumably motivated and reinforced by the same drive state and contingencies as was the original avoidance acquisition). For example, Coulter, Riccio, and Page (1969) noted that rats which received response prevention were more hesitant to reenter the shock compartment than rats which had been normally extinguished. Although the authors note the apparent contradiction with two-factor theory, they chose to explain the discrepancy by assuming that rats which had received response prevention had adventitiously acquired a substitute avoidance response that was responsible for the apparent extinction. Whenever the CS terminated during response prevention, any response that the subject was performing would be adventitiously reinforced.

Such explanations appear to be contrived especially in light of the previous contradictions between the data and two-factor theory. Yet, even if we ignore these other discrepancies in two-factor accounts, the argument that competing responses are adventitiously acquired through CS termination during response prevention cannot explain the cessation of avoidance responding when one-way procedures are employed that do not include a discrete CS other than the dangerous apparatus cues themselves. In such instances response prevention never provides CS termination, and competing responses could never be reinforced via CS termination. However, one study, which used a one-way procedure and which did not provide discrete CS termination during response prevention (Page & Hall, 1953), explained the acquisition of a competing response as being "reinforced by the absence of shock." Here is a very early suggestion that the avoidance response may be reinforced solely through the absence (or avoidance) of the UCS. This is not a two-factor explanation.

Before considering the notion that the avoidance contingency itself is primarily responsible for avoidance acquisition, it should be noted that those studies cited above, which measure fear of the CS by recording latencies to reenter the dangerous compartment, are severely confounded. If the acquisition of a competing response is postulated for whatever reason, that response will typically interfere with the response necessary for reentry to the dangerous compartment. It is thus difficult to interpret these latencies as being solely due to fear of that compartment. It is for this reason that those experiments that measure fear of the CS independently of apparatus cues are a more desirable procedure. In one such study conducted by Kamin, Brimer, and Black (1963) this was accomplished by means of a conditioned emotional response (CER) technique. Following various amounts of avoidance acquisition, avoidance extinction, or classical conditioning in a two-way shuttle box, the authors tested fear of the CS by measuring suppression of an appetitive bar-press response when the CS was presented in a different situation. They concluded that although a relationship does exist between the extent of avoidance extinction and the diminution of fear, the fact that subjects which had already attained a moderate degree of extinction were still fearful of the CS implied that other factors were operating to bring about avoidance extinction. Kamin et al. concluded that rather than extinction of avoidance being *caused* by diminished fear of the CS, it seems reasonable to conclude that prolonged extinction training provides the *opportunity* for fear of the CS to diminish. Their closing statement was: "The data on the whole reveal a considerable lack of parallelism between fear and instrumental behavior, and thus encourage speculation that variables other than fear of the CS are largely responsible for the maintenance of avoidance behavior" (p. 501).

At this point the consideration that the avoidance contingency itself is primarily responsible for a major part of avoidance responding seems very attractive. This is not a new notion. Page and Hall (1953) as well as other researchers (Bolles, 1972; Herrnstein, 1969; Herrnstein & Hineline, 1966) have suggested that the avoidance contingency is of prime importance in maintaining the avoidance response. However, Seligman and Johnston (1973) have presented the most detailed account of the data in the form of a cognitive theory of avoidance learning. These authors have cast their account in the nomenclature of outcome expectancies. However, these expectancies may be considered roughly equivalent to associations of the traditional type (especially since the conditions that promote associative formation are identical to those that produce expectancies; see Seligman & Johnston, p. 105). Their theory assumes that the relationship between responding and the presence or absence of shock is the most critical factor in determining avoidance behavior. They suggest that the subject forms expectancies that responding leads to no shock and not responding leads to shock (these expectancies are abbreviated respectively as $rE\bar{s}$ and $\bar{r}Es$). The most important implication of this cognitive theory of avoidance for the present topic is that an expectation that responding at time t (such as during the CS) will produce no shock ($r_tE\bar{s}$) and not responding at time t will produce shock ($\bar{r}_tEs$) can be formed independently of classical conditioning. In this way classically conditioned fear is not required to vary directly as a function of avoidance responding. This alleviates the problem of discrepancies between fear (either monitored autonomically or inferred behaviorally) and stages of avoidance acquisition or extinction. It also readily explains the efficacy of response prevention. As Seligman and Johnston state: "The efficacy of standard avoidance extinction procedures will, according to our theory, be a function of how likely the animal is to sample disconfirmation of $\bar{r}_tEs$. Anything that prevents an animal from producing r_t with total reliability should lead to more sampling and faster extinction" (p. 92). Similarly, the theory permits an explanation of why fear may remain following the extinction of the avoidance response through response prevention. "Our theory permits the avoidance response to extinguish during response blocking without extinction of the fear CR. Extinction of the fear CR occurs when enough CS–no-shock pairings have occurred for classical extinction. Disconfirmation of $\bar{r}_tEs$ also requires CS–no-shock pairings, but there is no reason why effective disconfirmation could not proceed at a faster rate than fear extinction" (Seligman & Johnston, p. 95). Hence, the simple assertion that avoidance responding occurs because the response avoids shock permits a clear explanation of the data.

It should be noted that the avoidance contingency argument claims the existence of two distinct learning events in the avoidance paradigm as does two-factor theory. But these are not the same events. Instead of acquiring one association between the CS and UCS and another between the avoidance response and CS termination, avoidance contingency explanations assume the formation of a CS–UCS association and an avoidance response – shock avoidance (or shock omission or no shock) contingency. According to the Seligman and Johnston (1973) theory, these contingencies (or associations) contribute to behavior in the avoidance situation independently of each other. Little interaction between these associations is specified (with the exception that skeletal CRs which develop from the CS–UCS contingency may in fact include the avoidance response and thus contribute to its acquisition). In essence, fear CRs that result from the CS–UCS association do not significantly affect the acquisition, maintenance, or extinction of the avoidance response. This does permit an explanation of the dissociation between correlates of fear and the occurrence of the avoidance response. The language of this assertion is also sufficiently clear to permit empirical verification. But it is questionable whether or not available techniques permit the requisite independent measurement of the emotional and cognitive components of avoidance behavior. To the extent that these measures are confounded (being affected by both emotional reactions and cognitive expectations) such verification becomes impossible.

The Monti and Smith (1976) study provides a prime example. If we are to accurately assess the role of the cognitive and emotional components in determining avoidance acquisition, maintenance, and extinction, then it is necessary that we develop techniques for *independently* measuring these components. If our measurement of the emotional component is confounded by the present state of the cognitive component or vice versa, one can never assess their roles in avoidance behavior. This sort of confounding was discussed in connection with the Page and Hall (1953) research. Is the latency to reenter the dangerous compartment a measurement of fear of the CS only, or is it confounded by competing response tendencies that are presumably determined by the prevailing cognitive expectations (or associations)?

We must ask the same question of the CER procedure. It is possible that suppression of the appetitive bar-press response is not solely a measure of *fear* of the CS but is also influenced by competing response tendencies. The CS in the CER paradigm may increase the tendency to perform the avoidance response because of the $\bar{r}_t$Es cognitive expectations. There is some hint that CER suppression ratio is a function of both reactions in the Monti and Smith (1976) experiment. According to the Seligman and Johnston (1973) assumption of independence of emotional and cognitive components of avoidance behavior, the amount of fear elicited by an equal number of CS–UCS and CS–no-UCS pairings, whether or not these pairings occurred in an avoidance paradigm or a classical conditioning paradigm, should be the same. Thus, if the CER procedure measured only fear of the CS then it should have revealed equal suppression ratios for Groups CD-B (classical defensive, blocked) and A-B (avoidance trained, blocked) and for Groups CD-NBHC (classical defensive, nonblocked in home cage) and A-NBHC (avoidance trained, nonblocked in shuttle box). Monti and Smith controlled for the number, duration, and sequence of CS–UCS and CS–no-UCS pairings for the pair of blocked and nonblocked groups. The data show that somehow the presence of the avoidance contingency lowered the suppression ratio for Groups A-B and A-NBHC in comparison to their respective classical conditioning controls. If the CER procedure measures only fear then the Seligman and Johnston assumption of independence of the emotional and cognitive components is incorrect. If the CER measures fear as well as the cognitive component (via the tendency of such a component to evoke competing responses) then the Seligman and Johnston account may be accurate, and the CER suppression measure is to be faulted for its lack of specificity. If both processes (emotional and cognitive) are independent of each other, as Seligman and Johnston suggest, the tendency of classically conditioned groups to show less suppression than avoidance-trained groups is explained by assuming that the avoidance-trained animals were as *frightened* of the CS as the classically trained animals, but their suppression ratios were augmented by competing response tendencies. A CS presentation for the avoidance groups lowers bar pressing because the animals are frightened *and* because they have a tendency to engage in the avoidance response. Without complete assurance that our measures reflect fear only or competing response tendencies only (developed through cognitive expectancies), one can neither prove nor disprove the Seligman and Johnston hypothesis nor make accurate conclusions about the nature of avoidance acquisition, extinction, or response prevention techniques. It is essential that uncontaminated measures of each component be developed. Measurement of autonomic states may provide the best means of assessing the emotional component, since they seem to be least affected by trained competing skeletal responses.

TECHNIQUES OF BEHAVIOR THERAPY

The lack of close concomitance between changes in fear states and cognitive expectancies either inferred behaviorally or assessed directly through verbal reports following implosive or flooding therapy with humans has been reported by several researchers. Riccio and Silvestri (1973) have reviewed several reports of this nature and generally agree with the thesis that emotional and cognitive components of human avoidance behavior can be manipulated independently. If there is any merit to this position one is immediately compelled to question the value of eliciting fear or anxiety in the therapeutic setting. It will be recalled that early experimental investigations of animal avoidance behavior led to the suggestion that fear must be evoked in order to extinguish Pavlovian associations to the CS and in turn remove the motivation and reinforcement for avoidance behavior. If the notion that cognitive expectancies lead to the occurrence of the avoidance behavior independently of anxiety, then it is quite possible that evoking fear is an unnecessary aspect of the implosive technique. It is possible that methods of desensitization also bring about changes in behavior by training the expectancy r̄Es̄ (not performing the avoidance response leads to no aversive event during this particular stimulus situation). The training in relaxation may merely prevent the onset of undue and nonessential excess amounts of arousal.

Of course, a measure of how much fear is evoked by a CS will vary directly with how rapidly that CS is extinguished. But this does not mean that the evocation of the fear causes the extinction. The fear may simply be an indication that the CS has been presented long enough to approach that *t* at which the expectation of shock is greatest. When inhibition of delay has developed to a CS of long durations, short presentations of that CS, although unpaired with a UCS, will not bring about extinction because the expectation of the UCS at time *t* (so many seconds or minutes of CS presentation) has not been disconfirmed (Seligman & Johnston, 1973, make this same argument). Therefore, it is to be expected that extinction will vary as a function of the amount of fear elicited, but only because the fear is an indication of the prevailing cognitive expectation. Rather than assuming that the anxiety that may be elicited in a therapeutic procedure is a causative ingredient in behavioral change, it may be more accurate to view it simply as an indication that expectations are strong at this point and therefore ripe for disconfirmation.

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Encoding the Levels of Information in Pictures and Words

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SUMMARY

From a levels-of-processing framework (e.g., Craik & Lockhart, 1972), we derive the idea that the levels of information implicit in a stimulus, such as its physical configuration, its name, and the category to which it belongs, may become available for subsequent processing at different times after stimulus onset. In particular, tasks which allow the use of physical codes should be performed more rapidly than those which require "deeper" information. There are two important implications here: The first is that the ability to use a code does not mean that that code has been "matched to" a representation in memory. The second is that "depth" effects (i.e., physical < name < conceptual) should be demonstrable within both pictorial and verbal materials. The approach as a whole may be contrasted with current dual-coding approaches (e.g., Paivio, Note 1), which, while they allow for different levels of meaning in both verbal and imaginal symbolic systems, seem forced to assume that certain types of conceptual information are more easily accommodated within the verbal system. This leads to the prediction that some kinds of conceptual information will not be *directly* available from pictures, but must instead become available to the subject via an interaction between the symbolic systems.

We tested these assumptions with a speeded-inference task (Van Rijn, 1973), which has properties that allow for unambiguous interpretation of reaction time differences across stimulus materials which require processing to the same explicit depth. Briefly, we found that pictures yield faster inferences than do words when the same *semantic* information is required for performance (Experiments 1 and 2), and that physical information is available sooner than conceptual information for both pictures (Experiment 4) and words (Experiment 5). Moreover, some types of pictorial materials (e.g., representations of proper nouns) function symbolically to the extent that they do not have an advantage in discriminability over words (Experiments 6 and 7), unless physical features are added to them which are redundant with the conceptual information the subject needs to perform (Experiment 8).

The results are best interpreted within a levels-of-processing framework, in which multiple codes or representations do not exist to be activated by the appropriate stimuli, but rather the stimuli themselves embody levels of information which are encoded and used as needed.

Hypotheses about the nature of knowledge often reduce to questions of representation; for example, In what form are experiences or skills stored such that they can be used for cataloging subsequent events? From this perspective, it follows that "new" events are recognized by matching them to stored events, so that hypotheses about stimulus or event encoding are also reducible to questions of representation. A more recent and somewhat different analysis of encoding derives from a levels-of-processing framework (Craik & Lockhart, 1972; Posner, 1969). According to this approach, any stimulus (or experience) allows encoding along a multiplicity of informational dimensions, ranging from purely physical (e.g., lines, angles) to strictly cognitive qualities implicit in the stimulus by virtue of its relation to a concept or conceptual category. In this view, the "stimulus-as-coded" is a flexible entity which depends on the operations that a subject is required to and/or does perform with the abstracted information. It is assumed, moreover, that both the subject and the experimenter may "tap into" the abstraction process at different levels. Thus, the emphasis is shifted to questions of a more pragmatic nature; for example, To what depth does a stimulus have to be analyzed in order to be meaningful or usable in the given context?

In the present paper, we are concerned with the levels of information that are encodable from pictures and words. We propose that a levels-of-processing analysis of encoding implies that (a) other things being equal, (e.g., task requirements, discriminability, etc.), deeper information should be equally derivable from pictures and words; but (b) when picture–word differences do occur, they are *encoding* phenomena which result because task requirements generally favor more discriminable or identifiable stimuli.

The concept of multiple codes existing as stored representations against which current input is matched must be distinguished from the concept that levels of information are implicit in the stimulus event by virtue of the fact that the stimulus is both a physical entity (and thus will have physical features and relations among those features) and an instance of a concept or conceptual category. The former approach is best exemplified in the work of Posner (1969, 1973) and others (Posner, Boies, Eichelman, & Taylor, 1969). In the traditional view, most or all of the dimensions along which a stimulus may be coded already exist in stored representations. The processing of all the informational dimensions begins at stimulus onset, and recognition or matching judgments rely on a "horse race" among the codes, with physical codes generally finishing sooner (i.e., physical codes "find" their representations sooner than do name or conceptual codes).

A subtly different levels-of-processing viewpoint is held by Craik and Lockhart (1972). They maintain that stimuli are processed successively (rather than in parallel) through physical, name, and conceptual stages and that memory representations are by-products of this encoding process. It makes little difference to the present argument whether one agrees with the notion that the codes are processed in parallel or whether, in fact, a serial model is correct; in either case, the order in which the codes become available is the same. The distinction between Posner and Craik and Lockhart which we wish to highlight is that within the latter approach, recognition does not logically entail a match with a memory representation. Instead, as the stimulus is processed through several levels, different types of in-

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formation become available for use in subsequent processing. The task a subject is faced with may or may not allow him to make use of a particular type of information.

The idea that different levels of information are implicit in the stimulus is derived from the latter framework. We believe that, in fact, the processing of all available stimulus information begins at stimulus onset; however, we do not believe that recognition entails a "memory match" or, for that matter, that recognition is an appropriate construct for many of the tasks in which it is assumed to occur. Tasks which allow the use of physical information will generally be performed faster than those which do not, regardless of whether stimuli are pictures, words, or three-dimensional objects, simply because the physical information is available as a code sooner than name or conceptual information. However, the latter information is implicit in the stimulus and its availability does not necessarily rely on having named the stimulus or having processed it as a bundle of physical features.

At this point, a digression is in order so that we may clarify what we mean by statements like "pictures may be identified sooner than words on the basis of physical discriminability." We believe that a stimulus is "identified" at a highly abstract level of meaning *prior to,* or possibly *simultaneously with,* the extraction of other information from it. Identification does not imply recognition, nor does it imply that such tacit knowledge is consciously accessible to the subject (see Turvey, 1974). A good analogy would be a computer programmer who is trying to get his program to execute a READ statement in FORTRAN which has mistakenly been formatted to expect real numbers; instead, it finds alphanumeric information and aborts. What is puzzling to the programmer is that the compiler seems to "know" some very general and logically more abstract information about the data *class* prior to being able to read and/or recognize the *specific* data input. For the present purposes, by *identification of the stimulus,* we simply mean that the subject knows, in the sense that the compiler does, which stimuli have been presented and may now proceed to encode those stimuli along physical, name, and conceptual dimensions. While this viewpoint is, of course, different from the Craik and Lockhart (1972) proposal that subjects encode successive levels of information from the outset, we believe that the theories are compatible to the extent that they both assume, as far as *availability* is concerned, that physical information will generally be available to consciousness sooner than name or conceptual information. Moreover, we agree with their assumption that memory representations of these different codes exist as by-products of the encoding process and are not necessary to the extraction of information from the stimulus.

Both our own views and the approach outlined by Posner (1969) and Craik and Lockhart (1972) are in turn different from a dual-coding approach. While Paivio (1971, Chapter 3) has explicitly allowed for multiple levels of meaning in both imaginal and verbal systems (specifically representational, referential, and associative meaning), he has also stressed the functional differences between the systems. For example,

> The relations among the functions vis-à-vis the two symbolic systems can be informally summarized as follows: imagery is relatively better than the verbal system for representing and coping with the concrete aspects of a situation, with transformations, and with parallel processing in the spatial sense. The verbal system is superior in abstract and sequential processing tasks. (p. 38)

Also,

> Thus imagery is assumed to be specialized for the symbolic representation of concrete situations and events, speed and flexibility of transformational thinking. . . and parallel processing in the visual-spatial sense. The verbal system, on the other hand, is presumably characterized by its capacity to deal with abstract problems, *concepts, and relationships,* and for processing sequential information. (p. 434; italics added)

Moreover, in order to save the image from the classic criticism that images can neither mediate nor represent abstract, generic knowledge, Paivio (Note 1) resorts to the argument that general ideas may be represented via specific exemplars which are in some sense prototypical of a class. Indeed, Rosch (1975) has shown that priming with a category name facilitates physical identity matches for good (i.e., prototypical) category members only. Importantly, she also found that while "basic objects" (e.g., dog, shoe, car) may indeed be representable by an "averaged shape" of all their particular instances, this is not the case at the level of superordinate (generic) objects (e.g., animal, clothing, vehicle) (Rosch, Mervis, Gray, Johnson, & Boyes-Braem, in press). In other words, we may image a generic *dog,* which combines the essential visual properties and shape of all dogs; however, we do not have a comparable image of *animal.*

What is important for the present argument is that if a dual-coding approach is to be viable, it must assume that superordinate generic information about instances of concepts is probably mediated via the interaction between the imaginal and verbal symbolic systems; an instance or picture of a dog "matches" its stored imaginal representation, which may in turn "contact" the corresponding representation in the verbal system, from which we may derive information such as its name, that it is an animal, that animals breathe, and so on. The implication is that certain types of conceptual information should not be *directly* available from pictures of specific instances and should at the very least require commerce between the two systems.

In order to test these proposals, we first need a task in which stimulus encoding is clearly separable from other operations. We then need to demonstrate levels-of-processing effects *within* both picture and word stimuli. Accordingly, this paper is divided into two sections: In the first, we present three experiments which validate a model for a task that allows us to separate stimulus encoding from subsequent operations and in which a picture–word difference is shown to occur in the stimulus encoding stage. In the second section, we present five experiments in which discriminability is constant among either picture or word stimuli and performance differences are found which are clearly attributable to different levels of processing required before task-relevant information is available.

ENCODING SEMANTIC INFORMATION FROM PICTURES AND WORDS

One mechanism that can conceivably produce performance differences across different levels of information (e.g., shape versus name versus category) is simply that deeper information is more difficult to encode or requires different and more complex procedures than does physical information. This relationship should hold for pictures as well as words; in particular, pictures should not need to be "recoded" in order to derive deeper information from them. In order to demonstrate this unambiguously, we need a task in which we can manipulate stimulus materials along dimensions which we assume require more or less analysis, in which the information required for performance is *explicitly* defined and constant across stimulus materials, and in which there is some objective way of determining that the stimulus manipulations affect encoding and not any other operations required for performance. The *same–different* task used by Posner (1969) and others to study processing time for various levels of information is deficient in the latter two requirements. First, in this task, the basis of the subject's match–no-match decision is not explicit and must be inferred from reaction time differences across stimulus materials. Second, there is no sure way of separating stimulus encoding from other possible mental processes involved in the task, and we need a task in which the stimulus-as-coded and the operations which use that code are separable.

Figure 1 shows a model for a task which we believe satisfies these requirements—an adapted version of the two-stimulus, subject-paced inference task (Daehler, 1972; Huttenlocher, 1962). In this task, the basis of the subject's response is explicit in the response itself, and there is a built-in check for the independence of stimulus encoding. To illustrate the task, suppose there are four stimuli which vary on color (red or blue) and form (circle or square) dimensions. We present a subject with two successive stimuli which have a common value on one dimension and a different value on the other (e.g., a red square and a red circle) and ask him to make an inference about which one of the four possible stimulus values is relevant. The correct value will depend on the predetermined assignment of each stimulus. For example, if we have told the subject that both stimuli contain the relevant value and are, therefore, positive instances (++), then a red square followed by a red circle require the response *red*. However, the same stimulus sequence requires the response *blue* if the subject has been told that neither stimulus contains the relevant value (− −). Similarly, we may have the subject make + − (square) and − + (circle) inferences. Van Rijn (1973) required his subjects to make these inferences as quickly as they could, and showed that when type of inference is a between-subjects variable, the order of difficulty (reaction time to solve correctly) is $++ < -- < -+ = +-$.

The model in Figure 1 is based on these data. It assumes that once the relevant characteristics (of geometric designs) have been abstracted from the stimuli, ++ subjects respond with the *value* common to both stimuli and −− subjects transform the common value to its complement and respond with the complement. We hypothesize that the only difference between ++ and −− conditions in Van Rijn's study is the additional mental operation that −− subjects must perform in the problem solving stage. All of the inferences or problem solving operations take place after the stimulus is encoded. We assume that the encoding stage involves the identification (as previously defined) of which two of the four possible stimuli have been shown and the subsequent abstraction of the characteristics necessary for solution. Note that while we are here assuming that the task may be divided into discrete stages (i.e., encoding and problem solving), the encoding of the stimuli cannot. We simply assume that at some point after presentation of both stimuli, the subject has the information he needs in order to proceed.

In the speeded-inference task, stimulus instances that are geometric designs require only superficial processing, since the solution to any problem is physically explicit in the stimuli. We wanted to design formally identical problems which would require a deeper level of analysis before solution-relevant characteristics would be available. To illustrate, suppose *hippo, mouse, bus,* and *car* are the instances, with attributes (LARGE or SMALL) and categories (ANIMAL or VEHICLE) as dimensions. The relevant values are characteristic of the stimuli but are not (necessarily) inherent in the stimulus configurations. The subject cannot determine the solution from any physical feature of either a picture or a word and must perform a deeper analysis to obtain the information he needs.

Suppose we presented these "semantic" problems with the instances represented as either pictures or words. The first thing to notice is that the subject must abstract and explicitly name exactly the same information (i.e., one of the four values) regardless of stimulus modality. Thus, comparisons can be made across stimulus materials with the information required for performance kept constant. The simplest expectation based on the model of Figure 1 and an analysis of task requirements is that reaction time will be the same for pictures and words. If re-

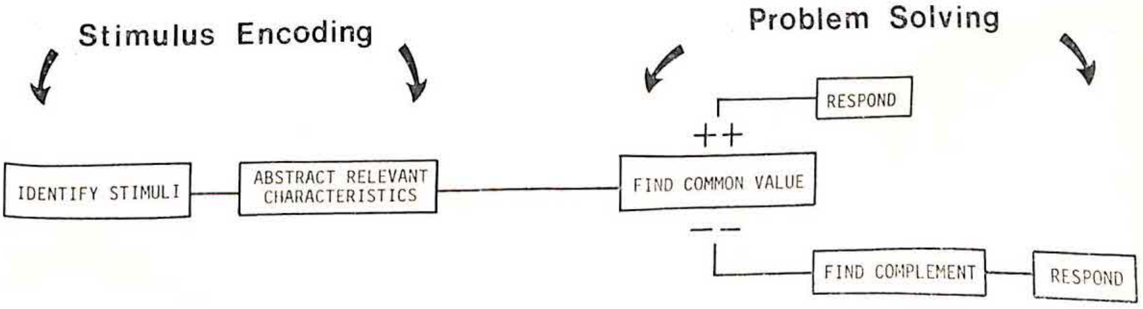

FIGURE 1. Process model for the speeded-inference task.

action time differences are observed, however, conclusions regarding encoding differences will be valid only if the difference in stimulus materials affects the encoding operations and nothing else. Our model for the task has a built-in check for whether the stimulus-as-coded can be separated from subsequent operations which use that code. Our argument is similar to Sternberg's (1969) demonstration that after practice in a memory search task, a visually degraded probe stimulus changes the y-intercept but not the slope of the reaction time function across different target set sizes. We propose that if changing stimulus materials (from pictures to words) affects stimulus encoding and not problem solving, *the reaction time difference between ++ and −− inferences will remain constant across materials.* This allows us unambiguously to assign differences between materials to stimulus encoding operations alone. On the other hand, if there is an interaction between stimulus materials and type of inference, then the stimulus-as-coded must somehow affect problem solving operations. If this is the case, we cannot unambiguously interpret a reaction time difference between pictures and words, and we would hesitate to argue that it arises because of differences in encoding operations per se.

If the reaction time difference between ++ and −− is constant, however, there are two hypothetical situations that can occur when subjects are required to abstract identical semantic information from either picture or word stimuli. The first possibility is that there will be no picture–word difference, and we would conclude that abstracting the same semantic information from either two pictures or two words requires similar if not identical processes for purposes of this speeded-inference task. This is the prediction of a literal levels-of-processing interpretation of stimulus encoding, since the task can only be completed after the stimuli are processed to the semantic level. The second possibility is that there will be a picture–word difference in reaction time. A "looser" levels-of-processing interpretation can account for the situation in which pictures are faster than words, using reasoning analogous to that used for the "CC vs. Aa" effect in Posner and Mitchell's (1967) *same–different* reaction time task. The effect is commonly attributed to the more rapid availability of physical information than name information. In our task, since the semantic information needed for correct performance is fixed and explicit across materials, a picture–word difference in favor of pictures can result only from physical differences between pictures and words which cause pictures to be more easily discriminated from one another. Once the stimuli have been discriminated or identified, determination of the relevant characteristics from them should take a constant amount of time. On the other hand, a dual-coding model (Paivio, 1975, Note 1), with its emphasis on differences in the *representations* of pictures and words, can more easily accommodate a reaction time difference in favor of words. This approach currently assumes that the verbal symbolic system is better suited for representing generic information, such as the category to which a specific event (e.g., a hippo) may belong. Thus, if words are faster than pictures, it seems reasonable to conclude that pictures and words are equally discriminable, but that semantic information can be derived directly from words, while pictures have to be recoded (i.e., the imaginal code must contact its verbal counterpart). Such a finding would also support a memory-match view of recognition. The experiments which follow were designed to try to distinguish among these alternatives.

For purposes of clarity throughout the paper, we use the following terminology for the independent variables. *Problem,* or *inference,* refers to a particular inference condition; except for Experiment 1, subjects made only ++ or −− inferences, and this was always a between-subjects variable. *Stimulus type* refers to all manipulations that result in stimuli which are different from one another *in kind* but may represent the same concept (e.g., pictures versus words, shapes versus names of states, words printed in color versus words printed in black ink, etc.). Within stimulus types, there are always only four unique *instances* (e.g., hippo, mouse, bus, and car). Two of these are shown for each *trial,* yielding eight unique trial types (e.g., hippo–mouse and mouse–hippo are two different trial types). The eight trial types map into four *relevant values* or *solutions* (e.g., in a ++ problem, hippo–mouse and mouse–hippo will both yield the solution ANIMAL), which in turn map into two *dimensions* (e.g., the relevant values ANIMAL and VEHICLE belong to the category dimension). The *relevant dimension* is the dimension which contains the relevant value for a particular trial type within a particular inference condition.

In Experiment 1, we replicated Van Rijn's (1973) study with semantic stimuli presented as either pictures or words. We included all four inference conditions, ++, + −, − +, and − −, to insure that the change from geometric designs to semantic stimuli did not affect the order of problem difficulty. However, we were mainly interested in the ++ and − − conditions, in which the likely processing strategies are clearer. We compared reaction times for both pictures and words across inference conditions in an effort to show that stimulus identification, as well as the abstraction of information from the stimuli, does not interact with subsequent processing stages.

Experiment 1

Method

Subjects and design. The 48 subjects who participated in the experiment each received $1.50. They were randomly assigned in equal numbers to one of eight conditions generated by the factorial combination of problem (++, −−, −+, and +−) and stimulus type (pictures or words).

Stimuli. A 2 × 2 matrix was constructed from attribute (LARGE or SMALL) and category (ANIMAL or VEHICLE) dimensions, and instances were found which exemplified the combination of relevant values for a given cell. *Hippo* was used for LARGE ANIMAL, *mouse* for SMALL ANIMAL, *bus* for LARGE VEHICLE, and *car* for SMALL VEHICLE. Thus, there were four trial types in which a category was common to both instances (e.g., hippo–mouse, mouse–hippo, bus–car, and car–bus), and four in which an attribute was common to both instances (e.g., hippo–bus, bus–hippo, mouse–car, and car–mouse). None of the solutions were physically present in the instances. For example, there was nothing in the *configuration* of either the word or the picture (an outline drawing) *hippo* which demonstrated that a hippo is a LARGE ANIMAL. This is a fact that the subject must know or determine from the configuration presented in order to perform the task.

Procedure. Van Rijn's (1973) procedure was followed as closely as possible. The subjects were seated in front of a rear projection screen. Under the screen were two lights which indicated the assignment of the stimulus (i.e., whether or not it contained the relevant value). After the nature of the problems and solutions were described to the subject, he had 8 practice problems, followed by 72 working problems. The first block of 8 working problems was not used in the data analysis, so each subject yielded a total of 64 data points. The eight trial types were randomized for each block of 8 trials.

A trial consisted of the following sequence of events: (a) The subject pushed a start button; (b) after a delay of about .5 sec, the first slide came on, causing a photocell to activate a millisecond timer. (c) When the subject pressed the button again, the timer was stopped, the second slide (from a different projector) replaced the first slide instantaneously, and a second timer was started. (d) The subject spoke his response into a Grason–Stadler voice key, causing the second timer to stop. (e) The experimenter recorded the two times and reset the apparatus for the next trial.

Results and Discussion

The rejection region for all of the following analyses is $p < .05$, and all MS_es are reported in milliseconds. The mean of both the first and second reaction times for each block of 8 trials was calculated for every subject, with error data omitted. Table 1 shows the mean reaction times and error rates for the four inference conditions; each mean is based on 192 scores (2 reaction times per block × 8 blocks per subject × 12 subjects per condition). The data from Van Rijn's study, using geometric stimuli, are included for comparison. Planned orthogonal contrasts showed that we replicated Van Rijn's essential results with semantic stimuli; the ++ inferences took less time than the − − inferences, $F(1, 44) = 15.56$; the + − and − + inferences were not reliably different from each other, $F(1, 44) < 1$; and the average reaction times to solve ++ and − − inferences were reliably faster than those to solve + − and − + inferences, $F(1, 44) = 17.96$, $MS_e = 359{,}281.53$. It is likely that the subjects in the latter two conditions used a much more heterogeneous set of strategies than did the subjects in the ++ and − − conditions; in any case, for present purposes,

TABLE 1

Mean Reaction Times and Percent Error in the Four Inference Conditions with Either Semantic (Experiment 1) or Geometric (Van Rijn, 1973) Solutions

Stimulus type		++	−−	−+	+−
Semantic	*M*	606.95	848.27	897.12	924.75
	% error	.78	2.6	2.9	2.5
Geometric	*M*	593.73	731.44	808.84	780.04
	% error	1.4	2.5	3.1	7.0

we intend to restrict the discussion to ++ and −− conditions only.

The ++ and −− data were analyzed in a mixed design, with problem and stimulus type (pictures versus words) as the between-subjects variables and reaction time (first or second) and blocks (1 to 8) as the within-subjects variables. The main effect of problem, $F(1,\ 20) = 23.83$, $MS_e = 234{,}605.49$, was expected on the basis of planned comparison. The main effect of reaction time, $F(1,\ 20) = 191.12$, $MS_e = 156{,}130.94$, indicates that first reaction times were always faster than second reaction times (449 versus 1006 msec, respectively). This is to be expected because in theory, the first reaction time reflects processing of the first stimulus, while the second reaction time covers both encoding of the second stimulus and any subsequent problem solving activities.

As required by a literal levels-of-processing interpretation, the effect of stimulus type was not reliable, $F(1,\ 20) = 3.59$, $.05 < p < .10$. The difference is large enough to give us some concern, however (the average second reaction times for pictures and words were 950 and 1062 msec, respectively). Importantly, there were no reliable interactions with stimulus type. Both the main effect of blocks, $F(7,\ 140) = 7.95$, $MS_e = 7{,}436.15$, and the Blocks × Reaction Time interaction, $F(7,\ 140) = 5.54$, $MS_e = 3{,}261.65$, suggested that practice might have either enhanced or diminished the picture–word difference, so we repeated the foregoing analysis on data from the last block of trials only. There were again significant effects of problem, $F(1,\ 20) = 22.88$, $MS_e = 28{,}041.91$, and reaction time, $F(1,\ 20) = 161.21$, $MS_e = 27{,}378.13$. In addition, a reliable main effect of stimulus type, $F(1,\ 20) = 4.60$, $MS_e = 28{,}041.91$, materialized, although once again stimulus type did not interact with any other variable. Table 2 shows the mean second reaction times for all stimulus type and problem conditions from both analyses. It can be seen that by the last block of trials, the relative difference between ++ and −− is virtually identical for pictures and words (280 versus 271 msec) and pictures are on the average 136 msec faster than words. Since the ++ and −− difference is constant across materials, we can unambiguously assign the picture–word difference to the stimulus encoding stage. Regardless of the type of inference, words require 136 msec more than pictures before their relevant semantic characteristics are available to be used in an inference. The data are thus in accord with a levels-of-processing analysis which assumes that pictures are identified faster than words on the basis of physical discriminability, but once identified, extracting semantic information requires the same amount of time for both stimulus types.

To get additional evidence for this argument, the data from the last block of trials were partitioned into trials which had either category (e.g., ANIMAL or VEHICLE) or attribute (e.g., LARGE or SMALL) relevant values. Recent dual-coding approaches (e.g., Paivio, Note 1) assume that superordinate categories may be represented via prototypical exemplars; on the other hand, information about attributes of specific instances, especially visual attributes such as size or color, may easily be stored within the imaginal system (Moyer, 1973; Paivio,

TABLE 2

Mean Reaction Times for Picture and Word Stimuli as a Function of the Type of Inference

Stimulus type	Experiment 1 (Second reaction time)				Experiment 2	
	All blocks		Block 8		Block 8	
	++	−−	++	−−	++	−−
Pictures	797	1.104	787	1.067	819	1.077
Words	925	1.200	927	1.198	967	1.206
Words − Pictures:	128	96	140	131	148	129

1975). Since *car* was the only one of the four instances which might be considered prototypical of its category, a dual-coding approach should predict that for picture stimuli, trials with the size dimension relevant should be responded to faster than trials with the category dimension relevant, while for word stimuli, there should be no difference between the dimensions. Accordingly, the partitioned data were analyzed in a mixed design with problem and stimulus type the between-subjects variables and reaction time and relevant dimension the within-subjects variables. The main effect of relevant dimension, $F(1, 20) = 7.12$, and the Relevant Dimension × Problem interaction, $F(1, 20) = 8.36$, $MS_e = 13{,}334.93$, were the only new reliable effects. Table 3 shows the second reaction times for the interaction. The dual-coding predictions were not supported; there is virtually no difference between category and attribute trials in the ++ condition for either pictures or words. In the −− condition, however, the category dimension took longer than the attribute dimension, and the difference was similar for both pictures (148 msec) and words (195 msec). Since the category–attribute difference was roughly the same across stimulus materials, but interacted with problems, the locus of the effect seems likely to be in the problem solving stage. One possible explanation derives from the fact that the attribute values we used were linguistic opposites, while the category values were not; this would facilitate the proposed complement-taking operation for −− subjects but would not be expected to affect the ++ subjects.

The results of Experiment 1 can be summarized as follows. First, since the relative reaction time difference between ++ and −− was constant *across* stimulus materials, with solution characteristics held constant and explicitly known, we can be sure that stimulus encoding does not interact with problem solving operations and that the picture–word difference in speeded inference resides in the stimulus encoding stage. Second, the relevant dimension interacted with inference type independent of stimulus modality, and we can therefore tentatively assign the category–attribute difference to the problem solving stage. Both of these results lend support to a levels-of-processing interpretation of stimulus encoding, in which encoding involves both tacit identification of the stimulus and abstraction of characteristics which will be relevant to the task at hand. Despite the fact that the present task explicitly required abstracting the same *semantic* characteristics from both pictures and words, there was a small but significant difference in the encoding time in favor of pictures. This suggests that there were differences in discriminability among the pictures which allowed them to be identified from among one another sooner than the words could be.

TABLE 3

MEAN REACTION TIMES FOR CATEGORY AND ATTRIBUTE RELEVANT DIMENSIONS AS A FUNCTION OF STIMULUS TYPE AND INFERENCE FOR EXPERIMENT 1 (BLOCK 8 DATA)

Relevant dimension	Pictures		Words	
	++	−−	++	−−
Category	777	1,140	917	1,303
Attribute	800	992	937	1,108
Category − Attribute:	−23	148	−20	195

Experiment 2

The purpose of Experiment 2 was threefold: First, we wanted to replicate the picture–word effect of Experiment 1. Second, the inferences we made about the data from Experiment 1 were based on the assumption that both stimulus encoding and problem solving operations were measured by the second reaction time (time from the onset of the second stimulus). To obtain a cleaner and more rigorous measure of these effects, we held the duration of the first stimulus constant in Experiment 2 and measured reaction time from the onset of the second stimulus. Third, we have argued that the picture–word effects in a speeded-inference task reside in the stimulus encoding stage. But since all subjects responded verbally, subjects who received word stimuli might have experienced a form of response interference, which would confound this interpretation. We tested this by replicating the ++ and −− conditions of Experiment 1, having half the subjects respond verbally and half respond by pressing buttons. The picture–word difference should vanish or reverse itself in the button group if response interference was responsible for it.

Method

Subjects and design. The subjects were 40 undergraduates, who participated for course credit; they were randomly assigned to one of eight groups formed by the factorial combination of problem (++ or −−), stimulus type (pictures or words), and response mode (voice or buttons).

Procedure. There were two changes in the procedure from Experiment 1. First, the "stimulus information" lights were eliminated because in Experiment 1 they were ignored by the subjects after the first few trials. Second, the interval from the onset of the first stimulus to the onset of the second stimulus was fixed at 750 msec for all subjects. Reaction time was measured from the onset of the second stimulus until the subject's response. The instructions were changed to accommodate these differences.

For the button group, four telegraph keys were arranged in a semicircle, approximately 2 in. from the start button and equidistant from each other. Each key was labeled with one of the four relevant values. The subjects in this group were instructed to keep their index finger on the start button until they were ready to respond. In all other respects, the procedures of Experiment 1 and 2 were identical, as were the stimulus materials.

Results and Discussion

The overall error rates were 2.8%, 3.1%, 2.0%, and 3.8% for pictures (++ and −−) and words (++ and −−), respectively. For comparability to Experiment 1, the mean score for the last block of 8 trials was calculated for every subject, with error data omitted. These data were analyzed in a between-subjects design with stimulus type, problem, and response mode as the factors. The error term for all of the effects was $MS_e = 28{,}478.99$. The main effect of stimulus type was reliable, $F(1, 32) = 6.76$, showing that pictures were again faster than words (948 versus 1,087 msec). This is a difference of 139 msec, which is almost identical to the difference (136 msec) found for the second reaction time data in the last block of 8 trials in Experiment 1. The main effect of problem showed that the ++ inferences were reliably faster than the −− inferences, $F(1, 32) = 21.67$, and the main effect of response mode showed that it was easier to respond vocally (951 msec) than by pushing buttons (1,084 msec), $F(1, 32) = 6.19$. None of the interactions approached significance. Table 2 shows the Stimulus Type × Problem data for the last block of trials from the present experiment, where it can be compared with the data from Experiment 1. It can be seen that in the present experiment, the difference between inference conditions is again comparable across stimulus materials (258 versus 239 msec for pictures and words, respectively). Furthermore, the fact that the reaction times are of the same order of magnitude lends support to our hypothesis about the second reaction times of Experiment 1. Lastly, since the picture–word difference held up across response mode and was in fact even somewhat larger in the button group than in the voice group (163 versus 114 msec, respectively), we can rule out response interference as the cause of this difference.

Experiment 3

When the data from our first experiment were partitioned into trials which required category responses (e.g., hippo–mouse: ANIMAL) and trials which required attribute responses (e.g., hippo–bus: LARGE), there was no difference in reaction times for the ++ problems, but the category trials were reliably slower than the attribute trials on −− problems. Since this effect was of the same magnitude for both pictures and words, it was tentatively assigned to the problem solving stage of the task. We proposed that finding the complement of the common value was facilitated on −− inferences that required attribute responses because the attribute values we used (LARGE–SMALL) were linguistic opposites, while the category names (ANIMAL–VEHICLE) were not. Experiment 3 was designed to test this hypothesis.

We constructed four conditions. Each involved two different stimulus matrices in which the relevant values of one dimension were two categories and the relevant values of the other dimension were two attributes. In the *Both* condition, the values of both the category and attribute dimensions were linguistic opposites; in the *Attribute-opposite* condition, only the relevant values of the attribute dimension were linguistic opposites. Similarly, in the *Category-opposite* condition, only the category dimension had linguistically opposite relevant values, and finally, neither dimension had linguistically opposite relevant values in the *None* condition.

According to our reasoning about the properties of the task, it will be inappropriate to make comparisons *across* conditions, since the different stimulus matrices involved may require different amounts of analysis to completely encode their particular instances. However, we may compare the time required to find the complement of the common value for the category and attribute dimensions *within* each of the four conditions, to show whether linguistic "oppositeness" was the factor underlying the original category–attribute difference. More specifically, we have hypothesized that the "time-to-complement" the common value is the only operation which distinguishes −− from ++ inferences; this time may be estimated for each dimension within a condition by subtracting the mean ++ from the mean −− reaction times for that dimension. If our hypothesis is correct, the time-to-complement category and attribute values should be equal in the Both and None conditions, since linguistic oppositeness is either present or absent from both dimensions. On the other hand, in the Attribute-opposite condition, the time-to-complement attributes should be shorter than the time-to-complement categories, and in the Category-opposite condition, the reverse should be true. This is the same as saying that for these conditions, a property of a particular relevant dimension interacts with inference type during the inference or problem solving process and not during encoding.

Method

Subjects and design. The subjects were 48 undergraduates, who participated in the experiment for course credit. The first 36 subjects were randomly assigned to one of the conditions formed by the factorial combination of problem (++ and −−) and the number and type of dimensions whose values were linguistic opposites (Both, None, Category opposite, and Attribute opposite). Our initially constructed Category-opposite stimulus matrices were extremely difficult, and because of the time required to get new stimulus materials, all 12 subjects in the Category-opposite condition were tested during the final week of the semester. For these reasons, 12 additional subjects were run during the following semester as a replication of the Category-opposite condition.

There were two different stimulus matrices for each condition; half the subjects received Matrix A first and half received Matrix B first. Instances were found for the cells of each matrix, again yielding eight trial types; four in which the category of the instances was the same (e.g., hippo–mouse, mouse–hippo: ANIMAL) and four in which the attribute was the same (e.g., hippo–bus, bus–hippo: LARGE). There were eight trials in a block, one of each trial type in a random order. All of the stimuli were presented as words. The stimulus materials are shown in Table 4.

Procedure. The subjects received one block of practice and then four more blocks of trials on each stimulus matrix in their condition. Otherwise, the procedure was identical to the voice group of Experiment 2.

Results and Discussion

The error rates were 2.1%, 3.4%, 2.0%, and 4.3% for Both, None, Attribute-opposite, and Category-opposite conditions, respectively. Within each condition, a mean reaction time was obtained for ++ and −− inferences in which either an attribute or a category was the relevant value, and the difference between these means (i.e., the time-to-complement that dimension) was obtained. These data are shown in Table 5.

The difference between the time-to-complement attributes versus categories was tested against the standard error of the difference between both inference types within a condition, combined across relevant dimensions. As predicted, the attribute dimension was "easier" than the category dimension only in the Attribute-opposite condition, $t(22) = 2.58$, and the category dimension was easier than the attribute dimension in the Category-opposite condition, $t(22) = 4.00$, and in the replication of the Category-opposite condition, $t(22) = 4.82$. There was no difference in complement-taking times between the dimensions in either the Both or None conditions.

In general, these results confirm the hypothesis that subjects making −− inferences locate the common value and find its com-

TABLE 4

Stimulus Materials for Experiment 3

Condition	Matrix A			Matrix B		
Both		SOLID	LIQUID		MALE	FEMALE
	WHITE	salt	milk	YOUNG	boy	girl
	BLACK	coal	coffee	OLD	father	mother
None		FRUIT	BIRD		ANIMAL	BOAT
	RED	apple	robin	SMALL	mouse	canoe
	YELLOW	banana	canary	FAST	cheetah	cruiser
Attribute opposite		TIME	CLOTHING		ANIMAL	VEHICLE
	LONG	hour	gown	LARGE	hippo	bus
	SHORT	minute	skirt	SMALL	mouse	car
Category opposite		SOLID	LIQUID		FRUIT	VEGETABLE
	BLACK	coal	coffee	YELLOW	banana	squash
	COLD	ice	beer	SMALL	grape	pea

plement and that if a dimension has relevant values that are linguistic opposites, this process is facilitated.

General Discussion

Taken together, the results of the first two experiments rule out a strict levels-of-processing interpretation, which would claim that pictures should have been equal to words simply on the grounds that the same depth of information was required from both. They also seem inconsistent with a dual-coding hypothesis, however, since words, which should have more direct access to semantic or generic information, were slower than pictures. They are consonant with an interpretation which says that the same semantic information should in fact be equally derivable from both pictures and words, but that pictures may contain physical information which makes them more discriminably different from one another (more readily identifiable) than are words. From this viewpoint, the picture–word difference in speeded inference is an encoding phenomenon which results from differences in identifiability. In other words, within a levels-of-processing framework, one may expect reaction time differences in favor of pictures when performance requires discriminating a stimulus from among a set of other possibilities before proceeding with the task. An example of this is the increase in reaction time obtained when the probe stimulus in a memory search task is visually degraded (Sternberg, 1969), since the only encoding operation necessary for successful performance is the identification (or perhaps recognition) of the stimulus. The speeded-inference task requires further stimulus analysis in order to proceed to subsequent nonencoding operations. It remains to be seen whether we can use this task to get reaction time differences in the abstraction of different depths of information *within* pictures and words. The general prediction is that with discriminability held constant, semantic or conceptual solution-relevant information should require longer encoding times than should physical information, regardless of stimulus modality.

Levels of Processing Within Pictures and Words

Experiment 4

In the present study, we attempted to demonstrate an artificial picture–word difference, using only picture stimuli, by varying whether the relevant values were physically present in the pictures. In other words, we tried to manipulate the depth of processing which would be required to abstract a relevant characteristic from pictorial stimuli for a speeded inference. Characteristics physically present in the stimulus should become available to the subject sooner than characteristics which are not physically present, but once available, should not influence subsequent processing stages. This results in the prediction that with the *same* picture stimuli, inferences with "perceptual" solutions will be faster than inferences with "conceptual" solutions, and the facilitation will be confined to the stimulus encoding stage of the task.

Method

Subjects. The subjects were 40 undergraduates, who participated for course credit, and were divided equally among the four experimental groups.

Materials and design. In an attempt to avoid confounding frequency or familiarity with depth of processing, we used the nonsense creatures pictured in Figure 2 as stimulus instances. Four of the eight possible trial types which can be created from these instances have either HORNS or TAILS as their solution. Since HORNS and TAILS are pictured in the instances, this dimension was a perceptual dimension for all subjects. The only difference between the perceptual and conceptual conditions was in what the subjects were to call the relevant values of the four remaining trial types. In the perceptual condition, the subjects were told that ROUND and SQUARE were the other relevant values; again, both of these values are physical properties of the instances. In the conceptual condition, the subjects were told that ALIVE and EXTINCT were the other relevant values; neither of these are perceptually inherent in the instances, and this is therefore a conceptual dimension. In other words, given a ROUND creature with HORNS followed by a ROUND creature with a TAIL, a perceptual subject making a ++ inference would respond *round,* while a conceptual subject would respond *extinct.* Given a ROUND creature with HORNS followed by a SQUARE creature with HORNS, both perceptual and conceptual subjects would respond *horns.* We are particularly interested in comparing ALIVE–EXTINCT trials to HORNS–TAILS trials within the conceptual condition, since this would enable us to see a depth effect within individual subjects. However, we predicted that all of the trials with perceptual relevant values (i.e., HORNS, TAILS, ROUND, and SQUARE) would yield relatively fast reaction times, while the ALIVE-EXTINCT trials would be much slower.

Procedure. Each subject was instructed with a picture similar to Figure 2, with the labels on the picture appropriate for his condition. The subjects were told about four nonsense creatures that had either HORNS or TAILS and were of one of two types (ROUND–SQUARE or ALIVE–EXTINCT, depending on the group). The remainder of the procedure was identical to the voice condition of Experiment 2.

Results and Discussion

The error rates were .80%, 1.4%, 2.3%, and 3.8% for the perceptual (++ and −−) and conceptual (++ and −−) conditions, respectively. The data from the last block

TABLE 5

MEAN REACTION TIMES FOR ++ AND −− INFERENCES AND THE TIME-TO-COMPLEMENT THE RELEVANT VALUE AS A FUNCTION OF THE TYPE OF RELEVANT DIMENSION AND WHETHER ITS VALUES WERE LINGUISTIC OPPOSITES

Relevant dimension	Inference	Attribute opposite	Category opposite	Category-opposite replication	Both	None
Attribute	++	881	1,151	1,088	919	933
	−−	1,236	2,424	1,862	1,298	1,378
I. Time-to-complement common atrribute:		355	1,273	774	379	445
Category	++	853	1,318	1,225	1,012	938
	−−	1,364	1,942	1,588	1,421	1,315
II. Time-to-complement common category:		511	624	363	409	377
Difference between complementing times [(I) − (II)]:		−156[a] (60.45)	649[a] (162.33)	411[a] (85.39)	−30 (69.31)	68 (58.21)

Note. Numbers in parentheses are standard errors of the difference between ++ and −− across both relevant dimensions.
[a] $p < .05$.

TABLE 6

MEAN REACTION TIMES TO MAKE ++ AND −− INFERENCES AS A FUNCTION OF THE DEPTH OF PROCESSING REQUIRED BY THE RELEVANT DIMENSION FOR EXPERIMENT 4 (BLOCK 8 DATA)

Inference	Perceptual condition		Conceptual condition	
	Round–Square	Horns–Tails	Horns–Tails	Alive–Extinct
++	732	778	679	1,070
−−	1,017	1,041	955	1,305
(−−) − (++):	285	263	276	235

of problems for each subject were partitioned into the two main dimension types for his condition (i.e., HORNS–TAILS and either ROUND–SQUARE or ALIVE–EXTINCT), and an overall mean was calculated for each dimension, with error data omitted. These data were analyzed in a mixed design with condition (perceptual or conceptual) and problem (++ or −−) as the between-subjects variables and dimension as the within-subjects variable. Dimension 1 was HORNS–TAILS and Dimension 2 was either ROUND–SQUARE or ALIVE–EXTINCT. Note that by combining the ROUND–SQUARE and ALIVE–EXTINCT times, we were *decreasing* the difference between perceptual and conceptual conditions. In spite of this, the main effects of condition, $F(1, 36) = 6.68$, problem, $F(1, 36) = 38.51$, $MS_e = 36{,}416.01$, and dimension, $F(1, 36) = 24.42$, as well as the interaction of condition and dimension, $F(1, 36) = 35.33$, $MS_e = 23{,}169.46$, were all statistically reliable. All of the effects except problem reflected the longer reaction times on trials when either ALIVE or EXTINCT was the relevant value in the conceptual condition (see Table 6). Moreover, within this condition, the reaction times on trials with HORNS or TAILS relevant were comparable to those of the perceptual condition as a whole. As further testimony to the robustness of this effect, by the last block of trials, 18 out of 20 subjects in the conceptual condition (both ++ and −−) had longer reaction times on their ALIVE–EXTINCT trials than on their HORNS–TAILS trials, with a range from 39–822 msec and a mean difference of 424 msec. As a contrast, in the perceptual condition, 13 subjects had longer reaction times for HORNS–TAILS trials and 7 had longer reaction times for ROUND–SQUARE trials; for these 20 subjects combined, the range of the difference scores was from 5–287 msec, with a mean difference of 103 msec.

Since the reaction time difference between ++ and −− inferences is again roughly similar for both dimensions within and across conditions (and comparable to the difference obtained in the first two experiments), we know that the dimension effects are an encoding phenomenon. Furthermore, since all subjects saw exactly the same stimuli, the reaction time differences among dimensions cannot be attributed to discriminability, but must be taken as a genuine levels-of-processing effect.

Experiment 5

In the previous experiment, we obtained depth effects with pictures by having some of the relevant values be redundant with physical features of the stimuli. If we could show an analogous effect with word stimuli, it would provide converging evidence for the notion that at least some picture–word differences can be attributed to the subjects' ability to use discriminating physical features in pictures which are not usually avail-

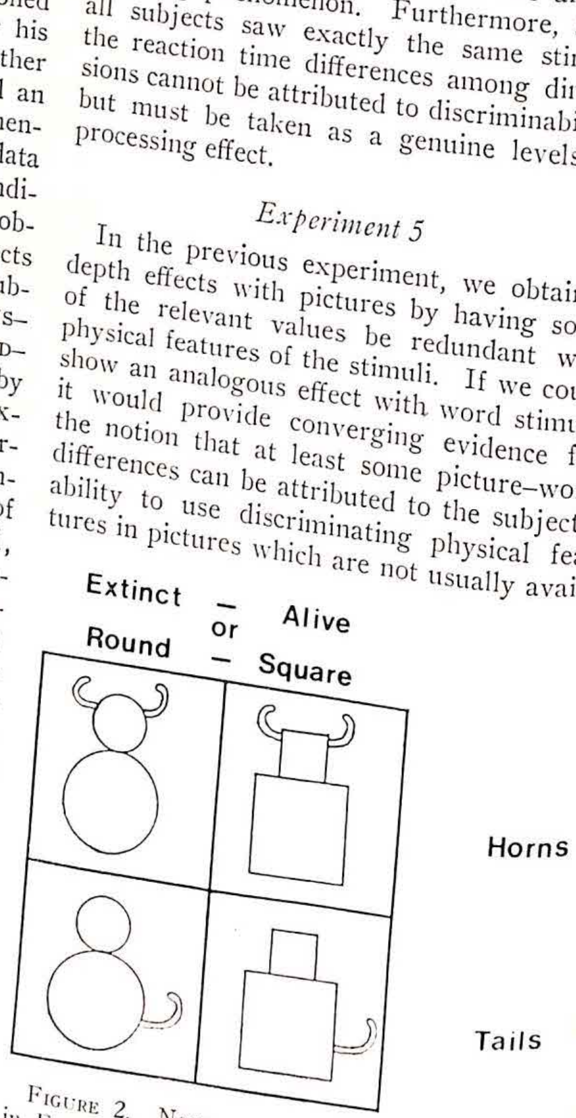

FIGURE 2. Nonsense creatures used as stimuli in Experiment 4. (The subjects were instructed with the use of a photograph similar to this figure except that only the values appropriate to their condition were displayed.)

able in words. The more that task-relevant processes can take advantage of physical attributes (e.g., in a visual search task [Paivio & Begg, 1974]), the more we should expect a picture–word difference. In the speeded-inference task, we can construct word matrices in which some of the relevant values are redundant with physical features of the *referents* of the words. For example, bananas and canaries, as *objects,* both share the property *yellow,* while apples and robins share the property *red.* Color is thus perceptually inherent in the referents and conceptually inherent in the names of the referents. However, if we print the stimulus words in yellow or red ink, then color becomes a physical feature of the words as well. "Normal" stimuli would presumably have to undergo more analysis than colored stimuli in order to derive the colors, in view of the fact that physical information should be available sooner than semantic information. Note that the subjects who receive colored stimuli cannot ignore the meaning of the words because a random half of their trials require category answers (i.e., FRUIT or BIRD) that the colors cannot cue. Thus, the important prediction from a levels-of-processing point of view is that RED–YELLOW trials will yield faster inferences when the stimulus instances are printed in color than when they are printed in black ink, and FRUIT–BIRD trials should not differ as a function of stimulus materials. The only real difference between Experiment 4 and the present experiment is in the stimulus materials; the logic is the same, insofar as trials with physical relevant values should yield faster inferences than trials with conceptual relevant values.

Method

Subjects. The subjects were 36 undergraduates, who participated for course credit, and were randomly assigned to one of four experimental groups obtained by combining problem (++ or −−) and group (C–N or N–C).

Materials and design. The stimulus matrix used for the present experiment was the same as Matrix A for the None condition in Experiment 3 (see Table 4). Half the subjects saw their instances printed in the appropriate color (either RED or YELLOW) and the other half saw them in black and white.

The subjects were instructed with index cards that showed the stimulus matrix appropriate for their group. All subjects received two practice and eight experimental blocks of trials with their original stimulus type (color or normal); a block consisted of one of each of the eight trial types in a random order (four with RED or YELLOW relevant and four with FRUIT or BIRD relevant). After this, they were transferred to the other stimulus type and run for four additional blocks, so that in a crude sense, each subject served as his own control. Group C–N (color–normal) received colored stimuli first and were transferred to normal stimuli, and Group N–C (normal–color) received normal and then colored stimuli. The eighth block of original trials was identical to the fourth block of transfer trials. The remainder of the procedure was the same as in Experiment 4.

Results and Discussion

The error rates were 1.7%, 2.7%, 1.1%, and 2.9% for Group C–N (++ and −−) and Group N–C (++ and −−), respectively. The data from the last block of both original and transfer trials were partitioned into trials on which either FRUIT–BIRD or RED–YELLOW values were relevant, and a mean reaction time was calculated for each dimension for every subject, excluding errors. These were analyzed in a mixed design, with relevant dimension and stage (original or transfer trials) as within-subjects variables and group (C–N or N–C) and problem (++ or −−) as between-subjects variables. The main effect of problem was reliable, as expected, $F(1, 32) = 151.15$, $MS_e = 42{,}167.58$. The main effect of stage $F(1, 32) = 4.17$, $MS_e = 18{,}572.95$, showed that the transfer trials were slower in general than the original trials (1,031 versus 985 msec), probably because the subjects had 10 blocks (including practice) with their original stimulus type and only 4 transfer blocks. The main effect of relevant dimension, $F(1, 32) = 25.88$, $MS_e = 21{,}493.06$, showed that the RED–YELLOW dimension was 124 msec faster than the FRUIT–BIRD dimension. Since the reaction time difference between ++ and −− inferences was roughly the same for both groups (414 versus 428 msec for C–N and N–C, respectively) and close to the difference obtained for this problem in Experiment 3 (411 msec), we can again assign differences across groups to encoding operations alone.

TABLE 7

Mean Reaction Times for Trials with Colored and Normal Stimuli as a Function of the Relevant Dimension and Stage of the Experiment for Experiment 5 (Block 8 Original Trials and Block 4 Transfer Trials)

Group	FRUIT or BIRD relevant		RED or YELLOW relevant	
	Original words	Transferred to	Original words	Transferred to
Color–normal	color: 1,026	normal: 1,127	color: 788	normal: 1,093
Normal–color	normal: 1,081	color: 1,045	normal: 1,045	color: 857
Normal − Color:	55	82	257	236

The most important results for the present hypothesis are the interactions that would be expected from fast reaction times on trials when either RED or YELLOW is the relevant value and the stimulus instances are printed in color, regardless of whether those trials were in the original or transfer stage. The Group × Stage effect, $F(1, 32) = 48.29$, $MS_e = 18{,}572.95$, showed that the colored stimuli were faster than the normal stimuli on the original trials (907 versus 1,063 msec) and on the transfer trials (951 versus 1,111 msec). The Group × Stage × Problem interaction, $F(1, 32) = 8.01$, $MS_e = 18{,}572.95$, showed that this relationship held for both ++ and −− inferences; and most importantly, the Group × Stage × Relevant Dimension interaction, $F(1, 32) = 16.92$, $MS_e = 16{,}672.92$, showed that the advantage for the colored stimuli was limited to trials when either RED or YELLOW was relevant, as predicted. Thus, the subjects who originally saw colored words (i.e., the Group C–N) made faster inferences on RED–YELLOW trials than on FRUIT–BIRD trials; when transferred to words in black ink, reaction time did not differ as a function of dimension. The reverse pattern was obtained for subjects in the Group N–C. Table 7 shows the data from this interaction.

We repeated the foregoing analysis on the RED–YELLOW trials alone in order to make sure that the predicted interaction (i.e., Group × Stage) is reliable for these problems. The results were essentially the same; the main effect of problem, $F(1, 32) = 81.56$, $MS_e = 36{,}344.27$, the Group × Stage interaction, $F(1, 32) = 45.24$, and the triple interaction, $F(1, 32) = 7.84$, $MS_e = 24{,}148.50$, were all reliable. Table 8 shows the triple interaction.

This experiment demonstrates, once again, that physical information is generally available sooner than conceptual or semantic information, and we should expect faster reaction times when the relevant dimension is physically present in the instances (as in Experiments 4 and 5) or when the physical characteristics of the instances allow them to have a "head start" in the encoding process (as in Experiments 1 and 2). While words are visual stimuli with perceptible physical features, the physical properties of words are not usable under most circumstances.

Experiment 6

The results so far suggest that depth effects can be obtained *within* picture and word stimuli, but that when pictures and words are both to be analyzed to the same depth (e.g., Experiments 1 and 2), pictures generally have the advantage of better discriminability, giving them a head start. It is reasonable, at this point, to wonder whether there can ever be a situation in which no picture–word difference occurs, as was originally expected.

The relationship between the name of an object and a picture which represents that object is usually one-to-many; that is, a single word is usually applicable to an almost infinite set of pictures which are referents of that word. Shapes of the United States (or countries and perhaps other stimuli) constitute a set of pictures which have the unique property of a one-to-one relationship between shape and name. Shapes of states may thus function at the same level of abstraction (i.e., the generic) as do names. If the shape of a state can serve as a symbol, it may be as identifiable as its name, leading to an expectation of no reaction time difference between shape and name. Alternatively, shapes of states may be experientially "rare" enough that their advantage in discriminability will be offset by the speed with which subjects can read their names. In the following experiment, we used names or shapes of states as stimuli and held responses constant at a relatively deep level in an attempt to demonstrate equal accessibility of conceptual information across these materials.

Method

Subjects and design. The subjects were forty undergraduates, who participated for course credit, and were assigned to one of the four groups made by the factorial combination of problem (++ or −−) and stimulus type (names or shapes). Except for changing the instruction card, the procedure was identical to the last two experiments.

Materials. Four of the United States—New York, Florida, Alaska, and Arizona—which can be uniquely characterized as being north or south and east or west were chosen. Two of the states were presented on each trial. Half the subjects in each inference condition saw the names of the states as stimuli, and half saw the outlines of their shapes. Thus, for example, a subject in the ++ condition who saw either the names or the shapes of New York and Alaska would respond with the location *north*. We felt that the geographical location of a state should be no more perceptually a part of its shape than of its name. That is, the location should be an attribute which is equally derivable from both name and shape, since it is inherent in the meaning or concept of the state.

There are again eight unique trial types, two for each geographical location (e.g., New York–Alaska, Alaska–New York: NORTH). A block of trials consisted of all eight types in a random order. On any given trial, a subject was shown two states and had to respond verbally and as rapidly as possible with the location they had in common (if he was in the ++ condition), or its opposite (if he was in the −− condition).

Results and Discussion

The error rates were 2.5%, 3.0%, 2.7%, and 3.3% for shapes (++ and −−) and names (++ and −−), respectively. A mean reaction time was calculated for the last block of problems for each subject, with error data omitted. The data were then analyzed in a 2×2 design, with problem and stimulus type the between-subjects variables. Only the main effect of problem, $F(1, 36) = 9.34$, $MS_e = 53{,}721.96$, was reliable. The mean reaction times for the ++ and −− problems were 973 and 1197 msec, respectively.

While the absence of a main effect of stimulus type (the mean reaction times to shape and name stimuli were 1027 and 1144 msec) is exactly what we predicted if the geographical location of a state was indeed the sort of information that required a deep level of processing to obtain and if shapes and names were functioning at the same level of abstraction, the within-subject variability was large enough to cause concern about these conclusions. When we calculated the mean score for each subject, for example, the difference in reaction time between the fastest and slowest subjects in each group was 634, 415, 926, and 852 msec for shapes ++, names ++, shapes −−, and names −−, respectively. Each subject seemed to reach a level of proficiency that might have been determined by some combination of his inference condition and stimulus type, but given that these were between-subjects variables, there was no way to be certain. We felt that a within-subject comparison of performance to name and shape stimuli would provide a stronger demon-

TABLE 8

MEAN REACTION TIMES FOR TRIALS WITH "RED" OR "YELLOW" RELEVANT AS A FUNCTION OF COLORED OR NORMAL STIMULI, TYPE OF INFERENCE, AND THE STAGE OF THE EXPERIMENT FOR EXPERIMENT 5 (BLOCK 8 ORIGINAL TRIALS AND BLOCK 4 TRANSFER TRIALS)

Group	++ Inferences		−− Inferences	
	Original words	Transferred to	Original words	Transferred to
Color–normal	color: 638	normal: 831	color: 938	normal: 1,355
Normal–color	normal: 798	color: 704	normal: 1,292	color: 1,011
Normal − Color:	160	127	354	344

stration that these two stimulus types provide equally effective access to solution-relevant information.

Experiment 7

As in Experiment 6, the purpose of Experiment 7 was to show that comparable performance levels obtain with names and shapes when the information required by the task resides in the meaning rather than in the physical attributes of those stimuli. We gave four subjects extended practice on speeded inference with the same materials used in Experiment 6, with all the trials of a given stimulus type being blocked, so that if it was possible to develop a rapid, perceptual-processing strategy with the shape stimuli, the subjects could do so. We predicted a steady decrease in reaction time across days, regardless of stimulus type, until asymptotic performance was reached. Stimulus type should not be a significant factor in determining a subject's final level of proficiency.

Method

Subjects and design. The task and materials were identical to those used in Experiment 6. Three volunteers and the first author had 10 blocks of trials with eight trials per block for 6 days. There were two subjects (one male and one female) in each inference condition. The stimulus type (names or shapes) was blocked by days such that during the first 3 days, one subject in each inference condition had name stimuli, while the other subject had shape stimuli; during the next 3 days, each subject was switched to the other stimulus type, after being appropriately instructed. In all other respects, the procedure was identical to that in Experiment 6.

Results and Discussion

The error rates, averaged across subjects, were 1.0%, 3.9%, 1.6%, and 4.4% for shape (++ and −−) and name (++ and −−) conditions, respectively. A mean score was calculated for each of the last eight blocks of each day. Thus, each subject yielded 48 data points; 24 for name stimuli and 24 for shape stimuli. The data were analyzed in a mixed design, with problem the between-subjects variable and stimulus type, days, and blocks the within-subjects variables.

Only the main effect of days was significant, $F(2,\ 4) = 25.56$, $MS_e = 19{,}705.53$. However, since there were only two subjects per inference condition, both main effects of problem and stimulus type were tested against only two degrees of freedom for error. We therefore decided to test both these effects nonparametrically via the Mann-Whitney U-test. A mean reaction time was calculated for each subject for each day, yielding 6 scores per subject (3 for names and 3 for shapes) and a total of 12 scores per inference condition. When the reaction times were rank ordered according to inference condition, then $U(12,\ 12) = 136$, which was statistically reliable. When they were rank ordered according to stimulus type, then $U(12,\ 12) = 80$, which was not significant. The mean reaction times to name and shape stimuli were nearly identical; in the ++ condition, the average reaction time for name and shape problems, respectively, was 748 and 750 msec; in the −− condition, the average reaction time was 1,069 and 1,067 msec.

Several conclusions about the results seem to be warranted by an inspection of the left side of Figure 3, which shows the relationship between stimulus type and problem over days. The reaction time performance of the subjects improved across days and was not a function of stimulus type. Moreover, the results support a levels-of-processing interpretation to the extent that the same depth of conceptual information was equally derivable from both pictures and words. They also appear to support the hypothesis that shapes of states function as symbols by virtue of their one-to-one relationship to a name.

Experiment 8

If the shapes of states really do function as symbols, then it should be possible to perform a manipulation analogous to what we did in Experiment 5, when we obtained faster performance with word stimuli whose built-in perceptual features were redundant with the responses. We should be able to add features to the shapes of states which

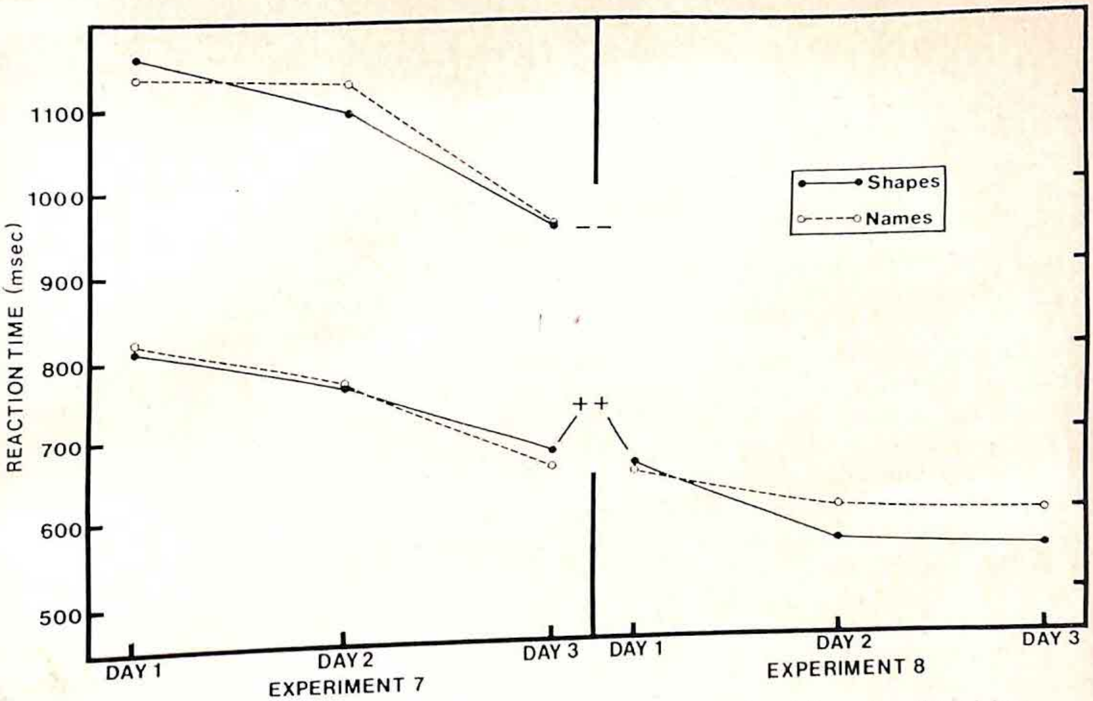

FIGURE 3. The Stimulus Type × Problem data for Experiments 7 and 8 as a function of days.

would allow them to be identified or discriminated from each other sooner than their names and therefore yield faster inferences.

Method

Subjects and design. The same four subjects from Experiment 7 were used in the present experiment; there was about a 6-week delay between experiments. All four subjects were tested in the ++ condition. Two subjects received 3 days of name stimuli and then 3 days of shape stimuli and the other two subjects received the reverse order. There were two practice blocks and eight experimental blocks per day. One subject in each order had previously been in the −− condition. The procedure was otherwise identical to that in Experiment 7.

Materials. We constructed a new population of shape stimuli, in which the western states (Alaska and Arizona) were approximately twice as large as the eastern states (New York and Florida), and the northern states (Alaska and New York) had a solid border, while the southern states (Arizona and Florida) had a dotted border. Thus, size and border were physical dimensions that were redundant with geographic location.

Results and Discussion

The error rates, averaged across subjects, were 2.7% and 3.4% for shapes and names, respectively. The data are shown in Figure 3; it can be seen that reaction time to name stimuli hardly improves over days, while reaction time to shape stimuli decreases. A mean score was obtained for each of the last eight blocks of trials on the third day for each stimulus type, with errors omitted. The data were analyzed in a mixed design, with blocks (1–8) and stimulus type (shapes or names) the within-subjects variables and order (shapes first or names first) the between-subjects variable. Both the main effect of stimulus type, $F(1, 2) = 29.71$, and the Stimulus Type × Order interaction, $F(1, 2) = 12.21$, $MS_e = 948.64$, were reliable. The main effect shows that shapes were reliably faster than names (555 versus 596 msec, respectively). The interaction is due to the fact that the two subjects who received shape stimuli first did not show any further improvement at all when switched to name stimuli (their mean reaction times for Day 3 shapes and Day 6 names were 582 and 597 msec), while the subjects who received name stimuli first were able to benefit from the perceptual features after the switch (their means for Day 3 names and Day 6 shapes were 595 and 529 msec).

The results of Experiments 7 and 8 imply that pictures can function like names whenever they are representations of unique (i.e., proper noun) objects. For present purposes, however, the important point is that redundant physical attributes which may be used as an aid in discriminating the stimuli from one another, in fact, produce faster inferences.

General Discussion

In these experiments, we have used a speeded-inference task to contrast memory-match models of encoding with a levels-of-processing analysis. We chose this task because statements about the depth of analysis required for performance are ambiguous when we are not sure of the basis for the response (e.g., the "CC < Aa" difference for a name match is about the same magnitude as the "color < size" difference for a physical match [Posner, 1969]), or when we cannot be sure that effects we are attributing to different depths may in fact result from an interaction between encoding and other operations (e.g., Thorndike-Lorge word frequency has a different effect on picture–word *same* judgments than it does on naming the pictures [Wingfield, 1968]). With a speeded-inference task, we can be certain of the outcome of the stimulus encoding process because the subject must explicitly name the basis of his response. Furthermore, the task has a built-in check for the independence of encoding and other processing operations.

In general, the data of these experiments speak against any theory which assumes that pictures and words have differential access to memory representations (e.g., imaginal versus verbal) which comprise different forms of information (e.g., spatial versus generic). Picture–word differences in reaction time, where they exist in a speeded-inference task, are traceable to differences in stimulus discriminability and not to differences in the kind of information or code the stimulus contacts most readily.

The clearest and probably most important implication in these data pertains to the relationship between reaction time and depth of processing necessary to abstract the stimulus characteristics required by the task. Once a stimulus has been identified, further informational analysis proceeds along whichever dimensions are relevant to the performance requirement. Information about physical features is generally available sooner than semantic or conceptual information, regardless of the modality of the stimulus. While it may be that different memory representations do exist for physical and conceptual information, it seems unlikely that these codes are differentially activated by pictures and words for purposes of this task. Rather, it would appear as though the stimuli themselves, regardless of their modality, embody different levels of information which are encoded and then used as required. Inherent in both pictures and words is information about the concept they represent, either as an instance (picture) or as a name (word).

Our analysis implies that encoding a stimulus first entails discriminating it from among the set of possible alternatives and then, as the task requires, further analyzing it along whatever dimensions (physical or semantic) are necessary. We must again stress that we do not wish our use of the term *identification* to carry with it the implication that the stimulus is named or matched to a memory representation; rather, identification is tacit. Variables which may aid the identification of a particular stimulus do so by allowing for better discrimination among all the stimuli in a set. For example, in Experiments 1 and 2, we found a picture–word difference in favor of pictures when stimuli had to be processed to the same *semantic level*. The picture–word difference in these experiments cannot be a depth effect because the response requirements (the solution-relevant characteristics) are the same for both groups. It is instead due to the fact that under most circumstances, pictures are more readily distinguishable from one another than are words, giving them a head start in the encoding process. Experiment 8 lends further support to this idea, since we were able to produce a reaction time difference by adding physical features to the shapes of states that were redundant with, but different from, the conceptual information required for performance.

TABLE 9

REACTION TIMES FOR ++ INFERENCES AS A FUNCTION OF THE DEPTH OF THE REQUIRED RESPONSES

Experiment	Stimulus materials	Response	Reaction time (++)	$(\bar{X}++) - (\bar{X}--)$
8 (Day 3)	pictures (states with size and border dimensions)	NORTH, SOUTH, EAST, WEST	555	—
5 (Block 8)	words (colored ink)	RED, YELLOW	638	300
4 (Block 8)	pictures (nonsense creatures)	HORNS, TAIL, ROUND, SQUARE	730	275
Van Rijn (1973)	geometric designs	RED, YELLOW, SQUARE, TRIANGLE	761	273
1 and 2 (Block 8)	pictures (hippo, mouse, bus, car)	ANIMAL, VEHICLE, LARGE, SMALL	811	262
7 (Day 1)	pictures and words (states)	NORTH, SOUTH, EAST, WEST	817	329
1 and 2 (Block 8)	words (hippo, mouse, bus, car)	ANIMAL, VEHICLE, LARGE, SMALL	952	251
4 (Block 8)	pictures (nonsense creatures)	ALIVE, EXTINCT	1,070	235

In Table 9 we list, in order of increasing magnitude, the reaction times for the ++ conditions of seven different experiments comprising a wide range of stimulus materials. While comparisons across experiments should be made with caution, the *range* of the reaction time difference between inference conditions is only 94 msec, which is, in the context of this task, truly remarkable. We may therefore appropriately consider the reaction time differences across stimulus materials to be "purely" due to encoding factors, and try to find some regularity within them which would explain the particular ordering obtained. Scanning down the "Stimulus materials" column gives us no particular clue to the ordering of the reaction times; pictures of objects, words, states, and geometric designs seem to be randomly interleaved with one another, and the same stimulus materials may be found at the beginning and the end of the list. It is only when we look at the "Response" column that the data begin to make sense. It is generally the case that responses or relevant values which correspond to physical properties of the stimuli produce shorter reaction times than do responses which reflect conceptual properties, *regardless* of whether the materials are pictures or words.

Experiments 4 and 5 explicitly examined the reaction-time–informational-depth trend reflected in Table 9, the former using pictorial materials and the latter using words. As we have seen, the effect, demonstrated within subjects, was extremely powerful. Identifiability (as previously defined) cannot be responsible for encoding differences because the stimuli were the same for all subjects within each experiment. Experiments 6 and 7 similarly demonstrated that the location of a state, which we consider to be a relatively conceptual property, is as derivable from its name as from its shape; this result is consistent with the present analysis and also with the notion that shapes of states may function at the generic (symbolic) level.

While performance differences to various types of stimuli are often taken as evidence for memory representations which contain qualitatively different kinds of information, this interpretation results from viewing the recognition process as a match between current input and stored representation. We have shown that by changing the required depth of processing, we can create performance differences within both pictorial and verbal materials. Within a levels-of-processing framework, stimuli are coded along whichever dimensions are relevant for performance, and the stored representations need not be contacted in order for the results of the current coding process to be available for subsequent use. The notion of *levels* of information derives from the idea that under most circumstances, physical codes are available for further use prior to name or conceptual codes. The fact that a subject can tap into his coding process at a variety of different depths suggests that recognition occurs at the point during encoding that is most appropriate for a given context.

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Item and Order Information in Short-Term Serial Memory

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SUMMARY

This article deals with memory for lists of items. First, the literature is briefly reviewed, and the main difficulties for traditional explanations of serial order effects are noted. The distinction between item and order information is suggested as an alternative way of explaining some of the standard serial order effects in the literature. However, the category effect may be a potential problem for an item-and-order model.

The category effect refers to the fact that a list composed of items from the same taxonomic category is better recalled than a list composed of items from different taxonomic categories, but at the same time shows more instances of transpositions (items recalled in the wrong position). To make possible recall–recognition comparisons, four experiments on recognition memory are reported. The general result was quite clear: With recognition, there was either no difference in the retention of order information between same-category and different-category lists or, in one case, same-category lists were actually better. Thus, the item-order comparison clearly works differently for recognition than it does for recall.

The results and the theoretical analysis seemed to support the general utility of the item–order distinction. A process model was described, but it did not seem too illuminating. A general finite state model can account for the category effects, but runs into a serious problem on the relative accuracy level of old and new items. A nesting model may be the most promising; it can account for item and order effects in general, though many details remain to be worked out. The article concludes with a general assessment of what we currently know about serial order effects in human memory.

One of the basic problems in human memory is how people remember a list of items. Many things in everyday life have a list structure—the days of the week, the months of the year, the spelling of words—and we routinely learn and remember many such lists. (A list is simply a set of items arranged in some particular order.) The laboratory analogues are serial order and memory span tasks, and these have been an important tool of the verbal-learning psychologist for most of this century. In fact, in his pioneering research Ebbinghaus (1913/1964) introduced the serial-learning task, and in their early work on memory Binet and Henri (1894) investigated memory span and serial position effects.

The first fact to be explained is that people can remember serial order information. With varying degrees of accuracy they do know how to spell words, they can learn a Shakespearean sonnet, or they can remember a telephone number long enough to dial it. Then, as has long been known, the accuracy of reproduction decreases as list length increases, increases with the number of repetitions, and is differentially affected by numerous experimental variables (e.g., presentation time per item, type of material, intraitem similarity, length and characteristics of the retention interval, and amount and nature of prior learning). Perhaps the most puzzling of all serial order effects is the well-known serial position curve: Beginning and end items are best remembered, middle items the worst.

The traditional explanation for serial order effects has been in terms of a chaining model, or item-to-item associations. This model suggests that there are direct links, or associations, from one item to the next, and these associations form the basis for both storage and retrieval. To learn or encode a serial string such associations must be established; to recall a serial string one reads off this chain much as one climbs a ladder. In climbing a ladder, each rung is first the target for the next step; once reached, it is then the stepping-off place for the next rung. So with a chain of associations: Each item is first a target or response to be made, then it serves as a probe or stimulus for the next item in the chain. To handle some of the empirical complexities, one assumes not only forward associations but also backward associations; there may be remote forward or backward associations (though see Slamecka, 1964), and these various associative connections may be graded in strength. This general view was first suggested by Ebbinghaus (1913/1964), and the most elaborate version is probably the mathematico-deductive theory of rote learning developed by Hull et al. (1940).

It now seems generally accepted that this view is at best inadequate, and perhaps even quite wrong. A critical account was given by Lashley (1951), who made several criticisms. One was that serial acts ran off at a higher speed than a chaining (or proprioceptive) view would permit; another was that such a view seemed inadequate as a basis for such rule-governed cognitive processess as understanding ordinary spoken language. Considerable experimental evidence against a chaining view was surveyed by Young (1968), particularly in connection with work on serial to paired-associate transfer. In this situation, one first learns a serial list, then a paired-associate list derived from the serial list. If the chaining view were correct, paired associates composed of items that had been directly linked in the serial list should be easier to learn than control pairs, but this does not seem to be the case.

In all fairness, the evidence here is not unambiguous. For one thing, many studies have reported positive transfer on the early trials in this paradigm, even though this benefit disappears by the time a criterion, say, of one perfect trial is reached. For another, the failure to obtain positive overall transfer may reflect certain peculiarities of

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the second task condition; Postman and Stark (1967) and Stark (1968) have reported positive overall transfer using what they feel to be more appropriate task conditions.

However, obtaining positive transfer in a serial to paired-associate task would seem to be a necessary but not sufficient condition for accepting a chaining hypothesis. If the chaining view were correct, positive transfer should be obtained. Any theory of serial ordering could probably easily be made to predict such an outcome, so this transfer test is a very weak test indeed. The general lack of enthusiasm for a chaining model is probably based on the fact that it does not seem like a very powerful model.

The other traditional interpretation of serial order effects is in terms of item-to-position associations. Assume each item is associated not to the next item but to some ordinal representation of its position in the list. Then serial learning by the anticipation method of performance on a memory span task could be mediated by these ordinal associations. Much as one reads or writes an array in computer programming, one somehow increments the subscript for each item encoded or retrieved; these item-to-position associations are implicit in any serial order task. Failure to find positive serial to paired-associate transfer would not be too embarrassing to this position. It could reasonably be argued that the subject preferred to directly associate items to each other in the required paired-associate task, so whether the item subscripts were appropriate or inappropriate was really immaterial.

Tests of this view have centered around the search for the stimulus in serial learning. The evidence seems somewhat equivocal: While there is some support for a relative position hypothesis, there are some inconsistencies as well (Harcum, 1975; Young, 1968). However, perhaps the main conclusion reached by Young (1968) in his review article is worth repeating; namely, the serial-learning paradigm poses considerable problems for those interested in understanding the basic processes of serial ordering. It is perhaps for this reason that interest in these problems has shifted to the memory area rather than the verbal-learning area. The memory effects may be simpler and easier to understand.

In the memory area, there are counterparts to the two views discussed above. Wickelgren (1974) has distinguished between associative and nonassociative memory. The distinction is very much like the distinction between the chaining view and the item-to-position view, though the notion of associative memory has the additional implication of being a content-addressable store. The bin model of Conrad (1965) is strictly a nonassociative memory, where items are located in numbered storage registers; thus, it is a model based on item-to-position associations, though cast in a somewhat different language. Estes (1972) has proposed a structural model wherein the associations are between control elements and items, with hierarchical arrangement to explain the grouping or chunking effects that can easily be demonstrated with short serial lists.

Orthogonal to all these models is the view that memory for serial order information can be dissected into two components: one for item information and one for order information. One can remember an item from a list without remembering (or with some uncertainty about) its ordinal position; conversely, one can remember that something was presented in the fourth serial position without being quite sure what it was. The distinction between item and order information was formally represented in a model by Crossman (1961) and was useful in explaining the "magic number seven" puzzle described by Miller (1956). If memory is part of an information-processing system, why does memory span vary with stimulus material when capacity (i.e., span) is assessed by the standard information theory measure? The Crossman answer to this puzzle was that if item and order information are measured separately, invariance may be the case after all.

There seems to have been little in the way of theoretical elaboration of the Crossman (1961) model since its introduction. Instead, the interest in this question seems to have been more empirical than theoretical. Murdock and vom Saal (1967) found a category effect; namely, that category similarity had a differential effect on item and order information. Using the Brown-Peterson distractor technique, they found that trigrams from the same category (e.g., GRAY BROWN RED) were more often recalled than trigrams from three different categories (e.g., EEL SCREW LOUNGE) but also were more likely to transpose (i.e., be recalled in the wrong order). By looking at conditional probabilities (the probability of an item error given the presence or absence of an order error), it was suggested loss of order information both preceded and increased the probability of loss of item information.

More recently, Donaldson and Glathe (1969) used signal detection measures to separate retention of item and order information in a short-term recognition–memory task. They found that the receiver operating characteristic (ROC) curves for order information agreed quite well with the standard detection theory measure. Healy (1974) reported a different experimental method of separating order and item information; she was able to obtain separate measures of order-only and item-only information. Her results led her to suggest that order and item errors are caused by two different processes.

Fuchs (1969) reported data which suggested that order errors showed a bow-shaped serial position curve, but item errors did not. Estes (1972) reported similar order error effects, which he called "distance" functions; that is, if a list item was given as a response, where did it occur? The distance function for end items were highly skewed, but middle items tended to be symmetrical. Distance functions show that there are gradient or diffusion effects for loss of order information. The existence of serial position effects can also be seen in the a posteriori probability curves. With an N by N input–output matrix, if an item was given as a response, what is the probability that it was given in the correct (input) position? Such data have been reported by Murdock (1968) for various probe techniques; there too, bow-shaped functions were obtained. Rohrman and Jahnke (1965), Zimmerman and Underwood (1968), and Detterman and Brown (1974) have all provided evidence that experimental separation of item and order information is possible.

Thus, it is quite clear that measures of order information can be devised, and these measures suggest that order information may have some different properties from item information. Bjork and Healy (1974) reported data which suggested that item and order information might be independent. They used acoustic similarity as their experimental lever and came to the opposite conclusion from that of Conrad (1965). Conrad (1965) had earlier suggested that loss of item information preceded loss of order information and that loss of item information would be particularly damaging if the stimulus materials were acoustically similar letters. However, as noted by Murdock and vom Saal (1967), the same transposition patterns occur with words as with letters, and no acoustic-similarity pattern is evident; consequently, the role of acoustic similarity cannot be crucial. The Murdock and vom Saal conclusion was the reverse of Conrad; namely, that loss of order information was more likely to precede loss of item information rather than vice versa. (Fozard, Myers, & Waugh, 1971 have also suggested a model in which loss of order information is based on loss of item information.)

In this article I pursue the notion of item and order information a bit further. The category effect and its implications for the distinction between item and order information are discussed. A finite state model is shown to be capable of solving some of the problems raised by the category effect. Four experiments on the recognition of order information are presented; these are an attempt to find out more about the locus of the category effect. Problems with a finite state model are discussed, and an alternative is suggested. The article concludes with a summary of where we now stand on our current understanding of item and order information in serial order memory.

THE CATEGORY EFFECT

Suppose we take the Crossman (1961) view that item and order information can be separated. Further, take the conclusion of

Murdock and vom Saal (1967) and Estes (1972) that loss of order information is primary and loss of item information is secondary; that is, loss of order information comes first and leads to loss of item information. If this is so, then the category effect poses an immediate puzzle. The category effect is that items from the same category are better remembered (their item information is remembered better) than items from different categories, but they transpose more (their order information is remembered worse) than items from different categories.

The puzzle is very simple. If loss of order information is primary and loss of item information is secondary, how can a condition (same-category items) that shows more loss of order information also show better retention of item information? This outcome would seem immediately to discredit the initial assumption, since one would expect item and order information to be correlated across conditions. Once a negative correlation is found (item information going one way, order information the other) the assumption that loss of order information causes loss of item information is hard to defend.

This puzzle is more apparent than real. It fails to distinguish a correlation within conditions from a correlation between conditions (see e.g., Mandler, 1959). Thus, for any specific set of material (e.g., same-category or different-category items) it should be the case that, as loss of order information increases (say, by increasing the duration of the retention interval), loss of item information should also increase. In fact, this seems to be the case; see Tables 1 and 3 of Murdock and vom Saal (1967). It need not be the case that a condition which transposes more is worse remembered; the base rates may be quite different.

Let me be more specific and give a concrete model in which it is true that: (a) loss of order information precedes (and increases the probability of) loss of item information, and (b) a condition that shows more transpositions (loss of order information) also shows less loss of item information. Imagine a simple three-state Markov model such that, if an item is in State IO then both item (I) and order (O) information are retained, if an item is in State I then item information is retained but order information has been lost, and if an item is in State N, the null (N) state, both item and order information have been lost. A simple state diagram is shown in Figure 1, and the transition matrix T is:

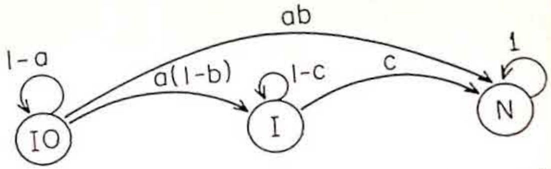

FIGURE 1. State diagram for finite state model. (In State IO both item and order information are retained, in State I only item information is retained, while in State N neither item nor order information is retained.)

$$T = \begin{array}{cc} & \begin{array}{ccc} \mathrm{IO} & \mathrm{I} & \mathrm{N} \end{array} \\ \begin{array}{c} \mathrm{IO} \\ \mathrm{I} \\ \mathrm{N} \end{array} & \left[\begin{array}{ccc} 1-a & a(1-b) & ab \\ 0 & 1-c & c \\ 0 & 0 & 1 \end{array}\right] \end{array} \qquad (1)$$

The parameter a gives the probability that an item will leave State IO; if it does, b is the probability that item information as well as order information will be lost. The parameter c is the probability that loss of item information will occur given that the item is in State I (i.e., that order information has already been lost).

In order to predict recall data one needs a vector for the starting state and a vector for the response rules. Call the former s, and it characterizes the probability distribution for the starting states. Assume

$$s = [p \quad 1-p \quad 0] \qquad (2)$$

so with probability p an item starts in State IO and with probability $1-p$ it starts in State I. (The psychological rationale here might be that encoding an item at all establishes some sort of item information, and something more is required to encode its serial order too. The attentive subject would never miss an item completely, so there is no probability of an item starting in State N.)

We need three response vectors, one for the probability that the serial string will be recalled in any order (free recall), one for

TABLE 1

SAMPLE OUTCOMES FOR HYPOTHETICAL PARAMETER VALUES

Parameter	Category	
	Same	Different
p	1.0	1.0
a	.1	.2
b	.1	.9
c	.5	.5
sT^2r_1	.936	.666
sT^2r_2	.831	.644
sT^2r_3	.105	.022

the probability that the serial string will be recalled in correct order (ordered recall), and one for the probability of transpositions (the probability that items will be recalled in the wrong order). Call these r_1, r_2, and r_3 and

$$r_1 = [1 \quad 1 \quad 0] \tag{3}$$

$$r_2 = [1 \quad 1/n! \quad 0] \tag{4}$$

$$r_3 = [0 \quad 1 - 1/n! \quad 0] \tag{5}$$

where n is the number of items in the string and would determine guessing probabilities. Thus, for free-recall (r_1) items in either States IO or I would be correct, for ordered-recall (r_2) items in State IO would be correct with probability 1 but items in State I would only be correct by chance, whereas only items in State I would transpose (r_3). (It is to be understood that s is a row vector and r_1–r_3 are column vectors.) Also it is assumed that all items in a list are in the same state, so the model applies to the string not the item.)

Following initial presentation, as the retention interval increases the probability of any type of response is simply sT^mr where m is the number of opportunities for transitions to occur (perhaps the number of units of interference) and r is the appropriate response vector (Equations 3–5) for the response type. To demonstrate the point, a simple example is given in Table 1. Same-category and different-category trigrams (so $n = 3$) are assumed to have the same values of p and c but different values of a and b. The case illustrated is where two units of interference have occurred. Free recall (sT^2r_1) is higher for same-category trigrams, .936 to .666; ordered recall (sT^2r_2) is also higher for same-category trigrams, .831 to .644, but same-category trigrams transpose more, .105 to .022. Thus, here is a very simple example to demonstrate that it is quite possible to have a case where a condition that shows more order errors also shows better ordered recall and free recall.

The hypothetical results shown in Table 1 are similar in most respects to the real data obtained in the Murdock and vom Saal (1967) experiment. Furthermore, a simple Markov model of this type might be generally applicable to serial order effects, in particular those obtained with the Brown-Peterson distractor technique. Generally, the data are what Markov models usually produce: steep but negatively accelerated forgetting. Also, studies of the buildup and release from proactive inhibition fit very well into a Markov framework, since these effects too are steep but span only a few trials. Conceptually as well a Markov model seems applicable. Like age distribution models in biology (see Searle, 1966), forgetting in the Brown-Peterson paradigm surely reflects the changing states of the items most recently stored in memory.

It is certainly true that Markov models are not particularly popular right now; process models are all the rage. However, the current disenchantment with Markov models may reflect inappropriate applications more than a general unsuitability of this type of approach. It might be thought that the distance functions and the a posteriori probability effects would immediately discredit a Markov model, since they suggest that order errors are graded rather than all-or-none. However, performance on a serial order task is a function not only of the item in question but also the memory state of other items in the list. Consequently, one should be wary of rejecting a finite state model on the basis of intuition; it might well be that an all-or-none model could predict gradient effects in a serial order task.

Analytic expressions for this three-state model can easily be obtained by standard methods. One simply takes the eigenvalues or latent roots (which are 1, $1 - a$, and $1 - c$) and computes the eigenvectors by solving the characteristic equation; then $T^m =$

UD^mU^{-1} where U is the matrix of eigenvectors and D the diagonal matrix of eigenvalues. Expressions for ordered recall, free recall, and transpositions can then be obtained by using the appropriate response vector (Equations 3–5). Standard parameter estimation techniques (e.g., STEPIT or SIMPLEX) can then be used to fit the model to any set of data and obtain the usual measure of goodness of fit.

After fitting the model to several sets of data, one of its inadequacies soon becomes apparent, that is, its assumption about guessing. All-or-none retention of a list is assumed, so if a list is in State I the probability of correct ordered recall is assumed to be $1/n!$. However, if we know anything about serial order it is that a list is a string of items and these items differ. There is no place here for serial position effects.

Serial position effects could easily be incorporated into the model by assuming that the model characterizes each item in the list rather than the list as a whole. Thus, everything said so far about lists would now apply to each item in the list and the response data would simply be the combined probability. However, such a possibility raises questions about interrelationships. A number of possibilities open up, and it is not a trivial matter to explore them. Further information is necessary to select the best line of approach.

Useful information could be obtained by recall–recognition comparisons. Is the category effect the same when memory is tested by recognition as it is when memory is tested by recall? If it is, that would imply that the category effect has something to do with whatever encoding or storage processes are common to recall and recognition. If it is different, how does it differ? In particular, is the category effect like other empirical effects (e.g., availability of items, organizational factors in memory, or the comparison between intentional and incidental learning) that show up clearly in recall but are weak or nonexistent in recognition? If the category effect follows this pattern, then it might be that the category effect operates on the retrieval processes that are required for recall but not required for recognition. This theoretical issue is discussed further in the General Discussion section after we have presented the experimental evidence.

Experimental Studies

We already know about the category effect in recall (e.g., Murdock and vom Saal, 1967). For recall–recognition comparisons, we can either accept this result as given and simply look at recognition, or we can combine recall–recognition comparisons in a single experiment. There are advantages and disadvantages both ways. If one combines recall and recognition in one experiment then one will at least know whether the recall effects replicate, but there may be unwanted carry-over effects from one condition to another (see Poulton, 1973). If one uses recognition-only, one has a purer and more efficient method to study the issue in question, but recognition-only tests may allow the subject to use selective learning techniques or strategies that would not work if recall and recognition were combined in a single experiment.

Given these pros and cons, it seemed best to study the problem both ways. The first two experiments used recall and recognition in a within-subjects design. The first experiment used a standard Brown-Peterson distractor technique, as in the Murdock and vom Saal (1967) study. The second experiment used a modified memory span (MMS) method, which allowed more information to be collected in a single trial.

The third and fourth experiments used only recognition. They extended the recognition part of the first two experiments by explicitly manipulating the lures and by using all five permutations (ACB, BAC, CBA, BCA, and CAB) of the trigram ABC. The fourth experiment replicated the third experiment by intensive testing of a small number of subjects, and also added latency measures in order to get some suggestions as to what type of mechanism might underlie recognition.

Experiment 1

Method

This experiment studied the category effect with both recall and recognition using the distractor

TABLE 2

NO. OF WORDS CORRECTLY RECALLED AND RECOGNIZED IN EXPERIMENT 1 FOR RETENTION INTERVALS AND FOR SAME- AND DIFFERENT-CATEGORY TRIGRAMS

Trigram	Information	Recall			Recognition		
		$t = 3$	$t = 9$	$t = 18$	$t = 3$	$t = 9$	$t = 18$
Same category	Item	203	191	174	262	257	265
	Order	157	145	125	181	167	173
Different category	Item	176	149	115	254	254	257
	Order	132	115	91	159	165	176

Note. Order information requires correct position while item information does not. Maximum per cell = 288.

technique as originally developed by Brown (1958) and Peterson and Peterson (1959). On each trial a trigram was presented, which we call a same-category or a different-category trigram. In the former, all three items were from the same taxonomic class (e.g., GRAY BROWN RED) while in the latter the items were from three different taxonomic classes (e.g., EEL SCREW LOUNGE). Following a variable retention interval (3 sec, 9 sec, or 18 sec) subjects were tested either for recall or recognition, and this was postcued. That is, a subject did not know until the time of the test which type of test it was to be. Further, we tried to separate the retention of item and order information for both recall and recognition.

Each trigram was shown to the subject on an index card, and the presentation duration was 1.5 sec. An arithmetic distraction task was used; every time a new two-digit number was spoken (once every 1.5 sec) the subject had to report the running total. (These numbers were tape recorded to ensure constancy of presentation.) After the retention interval for that trial had elapsed the experimenter stopped the tape recorder. On a recall test, the subject had to write down the three words of the trigram in the correct (ordered) locations in an answer booklet. On a recognition test, the subject was shown six cards of which three were the old items and three were new items. The subject first had to select the three old items (a test of item information), and then had to specify their order (a test for order information). For same-category trials, all six items were from the same category. For different-category trials, all six items were from different categories.

Subject. There were 24 subjects, students of both sexes from the University of Toronto undergraduate subject pool.

Design. There were 48 trials per subject, half recall and half recognition. Each trigram was the same or different category, and there were three retention intervals. The basic design was 2 × 2 × 3 with four replications per condition, all within subjects. The order of conditions was randomized subject to the constraint that one trial of each type occurred in each block of 12 trials.

Material. The trigrams were the same as those used in the Murdock and vom Saal (1967) study, except that several additional items had to be added to fill out the design. The same criteria were used; namely, medium to low-dominant monosyllabic and unambiguous category nouns from the Cohen, Bousfield, and Whitmarsh (1957) norms. As in Murdock and vom Saal, two different sets of material were used. Items (e.g., GRAY) appeared equally often in the same-category and the different-category condition; also they were tested equally often by recall and by recognition. Over the entire experiment each item occurred equally often at each retention interval.

Results

The main results are shown in Table 2, which gives the frequencies of correct recall and recognition summed over subjects. The maximum per cell is 288 (24 Subjects × 4 Trigrams Per Subject × 3 Words Per Trigram). Frequencies rather than proportions are reported so the reader can more easily calculate conditional probabilities and totals. For recall, item scoring gives credit regardless of position; thus, it is a free-recall criterion. For recognition, item scoring refers to the number of words (out of six) the subjects correctly selected for that trial. For recall, order scoring required that the words be written down in the correct location. For recognition, order scoring gave credit only for those words assigned to the correct serial position.

The first result to note is that same-category trigrams were better remembered (item scoring) than different-category trigrams, both for recall and recognition. The total number of words correctly recalled were 568 and 440 for same-category and different-category trigrams, respectively. The total number of words correctly recognized were 784 and 765 for same-category and different-category trigrams, respectively. Although the recognition difference was much smaller than the recall difference, both were significant ($p < .05$) by sign tests. There was a 21–3 split for recall and a 16–5 (3 ties) for recognition.

Second, the category effect on order information was completely absent for recognition. The number of items correctly selected but placed in the wrong serial positions were 263 and 265 for same-category and different-category trigrams. (This information can be extracted from Table 2 simply by obtaining the difference between item and order scoring for same- and different-category trigrams.) The item totals were 784 and 765 for same- and different-category trigrams, so the proportion of items assigned to the wrong serial positions was .335 and .346 in the two cases. Thus, regardless of whether one considers the absolute or the relative number of order errors, the category manipulation seemed to make no difference.

Third and finally, the category effect on ordered recall was a bit ambiguous. The absolute number of items recalled in the wrong positions was about 40% greater (141 to 102) for same- than different-category trigrams, but this difference was not quite significant by a sign test (15–7 split with 2 ties, $p = .067$ one tail). Also, even though the absolute number of order errors was greater for same-category than for different-category trigrams, the proportion of all items recalled that were incorrectly placed was almost exactly the same (.248 and .232) for the two conditions. Thus, it could be argued that while more categorized items are remembered than noncategorized items, given that an item was recalled at all, the probability that it was correctly given in the correct serial position was not affected by the category manipulation.

Discussion

The main point that emerges from Experiment 1 is that the category manipulation had no effect on the retention of order information as measured by recognition. While item information is slightly better for same- than different-category trigrams, neither by a relative nor an absolute measure is there any difference in recognition. The recall situation, however, is not quite so clear. An absolute measure seems to show the expected effect, though the difference is not quite significant. The proportional measure does not. The next experiment was a second attempt to look at this same issue, and a different experimental paradigm was employed.

Experiment 2

Method

This experiment used an MMS method. That is, it was like a memory span method except that four small memory span sets were presented and then tested by recall or recognition. The test was postcued. Each memory span set consisted of three items, written on a single index card, and each card was presented for 3 sec. With four sets, the total presentation time was 12 sec.

For recall, the subject was to write down the four sets of three items each. He could write down the sets in any order he wished (otherwise the task was too difficult), but within each set he was to write down the three items in order, indicating omissions by dashes. There were no explicit guessing instructions.

For recognition, each trigram was tested twice, once in the old (i.e., presentation) order and once in a new (permuted) order. There were two types of permutations, ACB (the second and third items in the trigram were switched) and BAC (the first and second items were switched). There was at least one instance of ACB and BAC on each trial, but the same total number of each for both same- and different-category trials. The subject was informed that half the tests were old, half new, but was not told that each trigram would always be tested twice. The order of the eight recognition tests (one old and one new for each of the four trigrams) was randomly determined for each trial. The subject was to respond on a 6-point Confidence-Judgment scale going from − − − ("sure the order is new") to + + + ("sure the order is old").

All four trigrams on a trial were homogeneous as regards category type. That is, if it was a same-category trial there was one category per card, so the three items on each card were all in the same category. However, the categories for the cards were sampled without replacement. On different-category trials, the items on each card were always from three different categories. However, it was possible for the same category (though not the same exemplar) to be represented on more than one card. Subjects were tested individually, and each subject was given 36 trials in a single session.

Subjects. The subjects were volunteers, 24 University of Toronto undergraduate students of both sexes who were friends and acquaintances of the experimenters.

TABLE 3

d' VALUES FOR SAME- AND DIFFERENT-CATEGORY CONDITIONS OF EXPERIMENT 2 FOR LURES ACB, BAC, AND COMBINED

Condition	ACB	BAC	Combined[a]
Same category	1.66	1.34	1.48
Different category	1.59	1.32	1.45

[a] For combined, the data from lures ACB and BAC were pooled.

Design. The design was $2 \times 2 \times 9$ within subjects, with two category types (same or different), two test types (recall or recognition), and nine replications per condition. Test type and category type were randomly ordered over the session. The stimulus material was completely counterbalanced. Each specific category item appeared equally often (across subjects) in the four main conditions (category type by test type).

Material. This experiment made use of a new general-purpose categorized word pool, which has recently been constructed for experiments needing categorized lists. Most of the categories and exemplars came from the norms of Battig and Montague (1969) but were supplemented with material from the norms of Loess, Brown, and Campbell (1969) and, to a lesser extent, with the norms of Hunt and Hodge (1971). The intent was to have a categorized word pool with 32 categories and 32 items per category, where the category items were unique and unambiguous category exemplars. That is not to say they are all well-known exemplars; not everyone knows that cardamon and cumin are seasonings, or that tangelo and kumquat are fruits. (These are perhaps the most extreme examples; salt and pepper were also included, as were apple and banana—though not orange.) It was necessary to go outside the norms in some cases to complete the categories, the category dominance clearly spanned a large range (e.g., salt to cardamon), and there was no control on word frequency. The only restriction was on word length; no word or category label more than 12 letters long was included.

The categorized word pool was examined and culled by a number of knowledgeable and imaginative members of the Ebbinghaus Empire, and the end result can be called the University of Toronto categorized word pool. The 32 categories are house parts, body parts, ships, birds, fruits, tools, weather, elements, names of men, sciences, cities, musical instruments, kitchenware, automobiles, fish, sports, occupations, trees, clothing, diseases, topography, crimes, countries, flowers, animals, insects, colors, U.S. states, textiles, seasonings, vegetables, and clergy. A listing of the categorized word pool is included as an Appendix.

Results

For recall, same-category trigrams were better recalled and transposed more than different-category trigrams. When an ordered recall criterion was used, the number of trigrams correctly recalled was 283 and 212 for same- and different-category trigrams, respectively. (With 24 subjects, 9 replications per condition per subject, and 4 trigrams per subject the maximum possible would be 864.) When a free-recall criterion was used, the number of trigrams correctly recalled was 307 and 217 for same- and different-category trigrams, respectively. There were 24 and 5 errorless transpositions for the same- and different-category trigrams, respectively, where an errorless transposition is one where all three items of a trigram are recalled but in some wrong order. When words rather than trigrams are analyzed, the total number of transpositions was 110 and 60 for same- and different-category trigrams, respectively. Thus, regardless of whether one considers the absolute or the relative number of order errors, retention of order information was clearly better for different-category trigrams than it was for same-category trigrams.

The main results for recognition are shown in Table 3, which gives d' values obtained from the EPCROC program of Ogilvie and Creelman (1968). (These d' values are used simply as a convenient descriptive statistic for the confidence–judgment data obtained in this experiment; no particular theoretical commitment is implied by their usage.) The d' values were computed separately for each type of lure (ACB and BAC), and in the combined case the data were pooled. As can be seen, ACB lures were more discriminable than BAC lures for both same-category and different-category trigrams, but there was neither a main effect of category nor an interaction with type of lure.

Discussion

The recall data replicated the original category effect very closely. When items were from the same category, recall (by either method of scoring) was better than when items were from three different categories. Also, there were more misplaced items (hence, loss of order information), either by an absolute or by a relative measure.

TABLE 4

d' VALUES FOR SAME- AND DIFFERENT-CATEGORY TRIGRAMS OF EXPERIMENT 3 FOR LURES

Lure	Same category	Different category
ACB	.82	.85
BAC	1.02	1.07
CBA	1.22	.93
BCA	1.69	.96
CAB	1.92	1.47
ACB + BAC	.92	.95
CBA + BCA + CAB	1.54	1.07

The recognition data clearly substantiated the results of Experiment 1. The main point, of course, is that again a category effect in recognition failed to materialize. There was a difference between the two types of lures (ACB vs. BAC), but the comparison across category type was the same for both. It will be reconsidered after the next experiment.

Experiment 3

This was the first of two experiments to use only recognition. The MMS method of Experiment 2 was used; recognition results with the Brown-Peterson and MMS method seem quite similar, and the MMS method is more efficient as a data-collecting procedure.

Method

On each trial four trigrams were presented on slides, and each slide was shown for 3 sec. As in Experiment 2, the category manipulation was between trials, so each trial was homogeneous in that it contained only same- or different-category trigrams. The trigrams were tested in the presentation order, so what might be considered a functionally equivalent retention interval was used. That is, the first trigram presented and tested always had three intervening study trigrams, while the last trigram presented and tested always had three intervening test trigrams. If study and test interference are equal, then interference-wise, the retention interval was the same for all four trigrams in a set.

In every block of six trials 24 recognition tests were broken down as follows: 10 ABC, 4 ACB, 4 BAC, 2 BCA, 2 CAB, and 2 CBA. This scheme resulted in approximately equal number of adjacent (ACB, BAC) and remote (BCA, CAB, CBA) lures as well as approximately equal numbers of old and new items. The order of these 24 tests was randomized. The actual presentation of the test items was auditory; after the four slides had been shown, the experimenter tested the subject by reading out each test item and getting his response to each item. The same 6-point Confidence–Judgment scale was again used (from "sure the order is different" to "sure the order is the same").

Each subject was given 24 trials, blocked in groups of six. Each group of six were all same- or different-category trials, and these groups alternated in ABAB fashion. This order was counterbalanced over subjects, as were the items in the trigrams. There were two sets of material; if an item was in a same-category trigram in one set it was in a different-category trigram in the other set. Groups and sets were orthogonal (2×2).

Subjects. The subjects were 24 students from the University of Toronto undergraduate subject pool, and they were tested individually.

Material. The word pool used in Experiment 1 was used here.

Results

The main results are shown in Table 4, which gives the d' values for the different types of lures. These values were obtained from the EPCROC program (Ogilvie & Creelman, 1968), and the same old-item data were necessarily used in all analyses appearing in a given column. The main reasons for using a signal detection analysis are that the d' measure is an economical summary statistic and has a reasonable theoretical interpretation in the present case. The interpretation is that the d' measure reflects discriminability, and that some types of lures are more discriminable (have higher d' values) than others.

The main point to note about Table 4 is that the same-category lures are as discriminable or more discriminable than different-category lures for all lure types; there are no exceptions. To be conservative, the only statistical tests that were done were between category type for adjacent transpositions (ACB + BAC) and for remote transpositions (CBA + BCA + CAB). The difference between same-category and different-category trigrams was not significant for adjacent transpositions, $t(1{,}726) = .27$, $p > .05$, while this same difference was significant, $t(1{,}534) = 3.28$, $p < .01$, for remote transpositions.

Discussion

Again there was no evidence that recognition of order information is worse when items are in the same category compared to when they are in different categories. When the comparison was based on adjacent transpositions, there seemed to be no effect of category type but when remote transpositions were used as lures the results went the other way. Same-category lures were more discriminable from old items (had a higher d') than different-category lures.

The possibility of a selective strategy with this recognition-only experiment should be discussed. One possible strategy a subject might use to minimize the storage load would be to attend only to the first word in each trigram and ignore the other two. Then whenever the test trigram had the same first word (i.e., ABC and ACB) he would say "yes," otherwise "no." He could in fact achieve a very respectable performance in this way, as the only penalty he would pay would be a high false-alarm rate when the lure was ACB. For all other lures and the target, he would be correct.

Three points argue against this interpretation. First, the difference between ACB and BAC (the two adjacent lures) was evenly distributed across the six confidence judgments, not reflecting a disparity chiefly in the "yes" categories. Second, if this explanation were correct then the performance on the other four lures (BAC, CBA, BCA, and CAB) should be high and constant, and it was not. Third, most of the difference was among these four types of lures rather than between these four types of lures and ACB, which is again counter to the strategy interpretation.

Another possible strategy a subject could use would be to attend only to the first (or last) two words in a trigram, and disregard the third. This strategy would reduce the memory load by one third and, if successful, could result in perfect performance. There is of course no assurance that a subject can in fact gate out one of three words shown simultaneously and in close proximity to one another. However, even if they could, the category comparison would still be meaningful. The functional memory load would differ from the nominal memory load, but still it would be four same-category pairs versus four different-category pairs. (Subjects 1 and 4, on questioning after the experiment, reported that they had noticed this method would work. However, both claimed they had not used it.)

It is difficult conclusively to rule out all possible strategy interpretations, but the data suggest that at least the obvious ones are implausible. So, until it is necessary to believe otherwise, we shall assume that the pattern of results shown in Table 4 tells us something basic about recognition memory for order information, not simply something about the method the subject uses to cope with the task demands.

Experiment 4

The final experiment was an attempt to replicate the results of Experiment 3 with some intensive testing of a few individual subjects. Further, to get at least an indication of what a possible mechanism might be, a latency measure was included as well. As is not uncommon in studies of recognition memory, the length of time required by the subject to make his confidence judgment was recorded along with the confidence judgment itself.

What will these latency data tell us? They should help to decide between two rather different possibilities. How does a subject decide whether a test trigram is different from a study trigram? There might be a direct comparison between the memory trace of the presented trigram and some encoded version of the probe trigram. If this were so, the larger the difference the easier the discrimination and the less time it should take. Since in general the remote transpositions are more discriminable than the adjacent transpositions, they should be responded to more quickly.

On the other hand, suppose the subject performed some mental operation on either the stored memory trace or the encoded probe trace. Perhaps he "repermutes" them until they match. The more steps it takes the more sure he can be that an apparent discrepancy is not due to some small memory error. By this reasoning, since it would take more steps to make a match with remote transpositions than with adjacent transpositions, the opposite prediction results. That is, remote transpositions, even though yielding larger d' values, should actually have longer latencies than adjacent transpositions.

TABLE 5

d' VALUES FOR SAME- AND DIFFERENT-CATEGORY TRIGRAMS OF EXPERIMENT 4 FOR SUBJECTS AND LURES

Lure	Subject 1		Subject 2		Subject 3		Subject 4	
	Same	Different	Same	Different	Same	Different	Same	Different
ACB	2.45	2.56	2.10	1.65	1.38	1.26	3.50	3.86
BAC	1.34	1.12	1.73	1.83	1.43	1.32	3.91	4.05
CBA	2.28	2.20	1.97	1.62	1.91	1.68	2.79	2.82
BCA	2.24	2.51	2.11	1.71[a]	1.78	1.67	4.01	4.63[b]
CAB	2.56	2.14	1.96	2.27	2.06	1.68	3.80	4.57[b]
M	2.17	2.11	1.97	1.82	1.71	1.52	3.60	3.99

[a] Computed from yes–no.
[b] False alarms = $1/4n$.

Method

The main differences between Experiment 3 and Experiment 4 were as follows: Experiment 4 was run on a laboratory computer, conditions were randomized rather than counterbalanced, the categorized word pool was used, a few subjects were tested intensively, and latencies as well as confidence judgments were obtained. Originally the intention was to vary the presentation duration in accord with the subject's performance, slowing it down as he did poorly but speeding it up as he did well. Despite the use of a standard PEST algorithm (Taylor & Creelman, 1967), this attempt failed miserably. Even when presentation slowed down to 20 sec/trigram, performance did not improve sufficiently to achieve the desired performance level (75% correct).

The subject sat at the console of the computer (a PDP-12A) and saw the words displayed on the cathode-ray tube (CRT) in front of him. The computer graphics used a 25 × 25 dot matrix for each letter so the legibility was quite good. Each triplet was displayed for 2 sec, and there was a 100-msec off-time between each triplet. Following the fourth triplet, a row of asterisks was shown for 1 sec, then 1 sec later it was followed by the tests. Each test triplet stayed on the screen until the subject had responded, and the subject used a panel of six keys to signify his confidence judgment. The latency that was recorded was the time elapsing between presentation of each test triplet and the depression of the confidence–judgment key.

As before, all trigrams on a trial were either same-category or different-category trigrams. For same-category trials, four categories were sampled while for different-category trials 12 categories were sampled. Thus, in the latter case, there was never any duplication of category across the four trigrams. There were 64 trials on a session, 32 of each type. Categories and words within each category were randomly sampled on each trial. The randomization was done as follows: Beginning with the category with the largest number of items left, categories were accumulated up to 4 or 12, as required. The actual categories used were chosen with equal probability from among the set of most-eligible categories. Within each category, words were randomly sampled without replacement across each experimental session.

The test type of each trigram was randomly determined. With probability .5 the test was old (ABC); with probability .25 the lure was adjacent (ACB or BAC), and with probability .25 the lure was remote (CBA, BCA, or CAB). The two types of adjacent lures were equiprobable as were the three types of remote lures. The subjects were told that on the average half the tests would be old and half would be new, but they were not told about the specific breakdown of adjacent and remote lures.

Subjects. The subjects were four volunteers who were University of Toronto undergraduate students. All had been in at least one computer-controlled experiment before, but not one involving order information or the categorized word pool. Each subject was tested for 5 sessions of 64 trials each. Since each trial had four trigrams, there were 1,280 observations per subject.

Results

The confidence–judgment data are summarized in Table 5, which shows the d' value for same-category and for different–category trigrams for each subject and the five types of lures. There is clearly no difference between same-category trigrams and different-category trigrams. By a sign test, there is an 11–9 split. Or, the mean d' values, summed over subjects and lures, were 2.37 for same-category trigrams and 2.36 for different-category trigrams. Obviously, retention of order information as measured by recognition has nothing to do with whether the items in the trigram have been selected from one or three different taxonomic classes.

Since the category manipulation had no effect on performance, the data were pooled

TABLE 6

d' VALUES FOR SUBJECTS OF EXPERIMENT 4 POOLED OVER SAME- AND DIFFERENT-CATEGORY TRIGRAMS

Lure	Subject 1	Subject 2	Subject 3	Subject 4	M
ACB	2.54	1.87	1.32	3.66	2.35
BAC	1.21	1.77	1.37	3.96	2.08
CBA	2.35	1.80	1.80	2.76	2.18
BCA	2.41	1.94	1.72	4.47	2.64
CAB	2.29	2.10	1.86	4.17	2.60

over same-category and different-category trigrams, and the d' values are shown in Table 6 on a subject-by-subject basis. These data are useful in evaluating the confusability of the different types of lures, and such an analysis should be quite helpful in understanding the processes involved in the retention of order information. Unfortunately, the data are a bit idiosyncratic, and no simple generalization seems to characterize the results of all four subjects. Consequently, the best approach is probably to go by the mean values shown in the last column of Table 6 and hope that the individual variation does not follow any consistent pattern. By this reasoning, it would seem that the two remote transpositions BCA and CAB are most discriminable, that by and large adjacent transpositions (ACB and BAC) are less discriminable than remote transpositions (CBA, BCA, and CAB), but that the remote transposition CBA is the odd man here; it seems to behave more like an adjacent transposition (has a lower d' value) than like a remote transposition.

To analyze latencies, we compared mean reaction time for high-confident correct rejections for high- and low-confusable lures. The low-confusable lures were BCA and CAB while the high-confusable lures were ACB and BAC (except for Subject 1, where only BAC was used). The mean latencies were 1,541 msec and 1,688 msec for Subject 1, 2,004 msec and 2,020 msec for Subject 2, 2,575 msec and 2,324 msec for Subject 3, and 1,550 msec and 1,539 msec for Subject 4 for the low- and high-confusable lures, respectively. Summed over subjects, low-confusable lures had a mean latency of 1,892 msec while high-confusable lures had a mean latency of 1,871 msec. Thus, it would seem as if accuracy and latency are not very highly correlated in this particular recognition–memory task.

In further analyses of the latency data, it was very clear that latency and confidence judgment was closely related. Pooled over subjects and yes–no responses, mean latencies were 1,907 msec, 3,095 msec, 3,386 msec, 3,610 msec, 3,506 msec, and 1,666 msec for sure-no, probably-no, guess-no, guess-yes, probably-yes, and sure-yes responses, respectively. This trend was quite clear in the data of each subject individually, and pooling over yes and no responses did not seem to hide any underlying interactions. This same U-shaped relationship between confidence judgments and latency has also been reported for item recognition data by Mandler and Boeck (1974) and Murdock and Dufty (1972).

High-confident hits (sure-yes responses to old items) seemed to be faster than high-confident correct rejections (sure-no responses to new items). The mean latencies were 1,315 msec and 1,570 msec for Subject 1, 1,532 msec and 2,014 msec for Subject 2, 1,955 msec and 2,509 msec for Subject 3, but 1,613 msec and 1,597 msec for Subject 4. Thus, the first three subjects showed a big effect, but the fourth subject was 16 msec in the wrong direction (for him, correct rejections were faster than hits). It is also the case that Subject 4 had the highest d' value by a good bit; we don't know whether this is more than accidental, since only four subjects were used in the experiment.

Discussion

For the fourth time, the category effect failed to materialize in recognition. There was simply no d' difference whatsoever between same-category and different-category trigrams. There were differences as a function of the type of lure used on the recognition test, but again the category variable did not interact with the lures. The category noneffect clearly held for all lures tested.

A simple dichotomy between adjacent and remote transpositions does not seem adequate to account for the discriminability of the lures. Two factors seem necessary. One factor is the number of items that change place; by this criterion ACB, BAC, and CBA go in one group while BCA and CAB go in the other group. The second factor is the total distance moved. By this criterion ACB and BAC go in one group while CBA, BCA, and CAB go in the other group. Combining these two factors gives at least a rank order of discriminability (as measured, say, by *d'*); ACB and BAC are lowest, CBA is intermediate, while BCA and CAB are highest. To predict differences within these groups one would need to invoke some serial position factor.

The latency data seem a bit puzzling. Overall, there was no consistent difference as a function of the distinctiveness of the lures. That is, high-confusable lures had about the same mean latency for high-confident correct rejections as low-confusable lures. One could interpret this as an indication that discriminability has no effect on reaction time; also, that any repermutation to make a match either does not occur or takes very little time. One counter to this conclusion could be the claim that the difference in discriminability between the alleged low and high *d'* lures was not large enough to affect the latency data. Another counter would be the findings on hits versus correct rejections.

Why on the average do correct rejections take so much longer to make than hits? Averaged across all four subjects, the mean values were 1,599 msec for hits and 1,890 msec for correct rejections. This is a difference of nearly 300 msec, which is a large effect indeed in the reaction time field. In item recognition studies, yes–no differences (generally in the same direction) are more in the 30 msec–50 msec range. This comparison would suggest some more complex operation on the memory trace is required to make judgments about order information than about item information. Perhaps, as suggested, a memory trace that is discrepant from the probe must be rearranged before a decision is possible, and that is why correct rejections are so much slower than hits. While this is at least a possibility, it should certainly be considered tentative at this point. There were after all only four subjects in the experiment, and the one subject with the best performance accuracy-wise showed no latency difference between hits and correct rejections.

General Discussion

The main results from this series of experiments are clear and easy to summarize. They are that for retention of item information, same-category trigrams are better remembered than different-category trigrams regardless of whether the test is by recognition or by recall. For order information, same-category trigrams are worse than different-category trigrams when tested by recall, but in recognition there is little or no difference. Although one must always be wary of accepting the null hypothesis, after four attempts and the one non-null result in the opposite direction, it would seem safe to say that the category effect does not hold for recognition.

What do these results mean? At a very general level, they are certainly consistent with the view that item and order information are two components of serial order information. Even without recognition, since recall of item information goes one way and recall of order information goes the other way when the category variable is manipulated, it seems clear that order and item information are separable. When one adds recognition, and again finds different effects of the category variable for item and order information, it would seem quite clear that any complete model of serial order effects cannot afford to overlook the distinction between retention of item information and retention of order information.

One possible explanation of these results could be in terms of the structural model of Estes (1972). This model is based on a reverberatory-loop principle, and loss of order information is assumed to precede loss of item information. As this is presented in terms of a computer simulation, we have tried to do a simulation ourselves to study some of its properties. Perhaps we have misunderstood, but it does not seem sufficiently unambiguous so that we feel our interpretation is completely correct. Consequently, it is

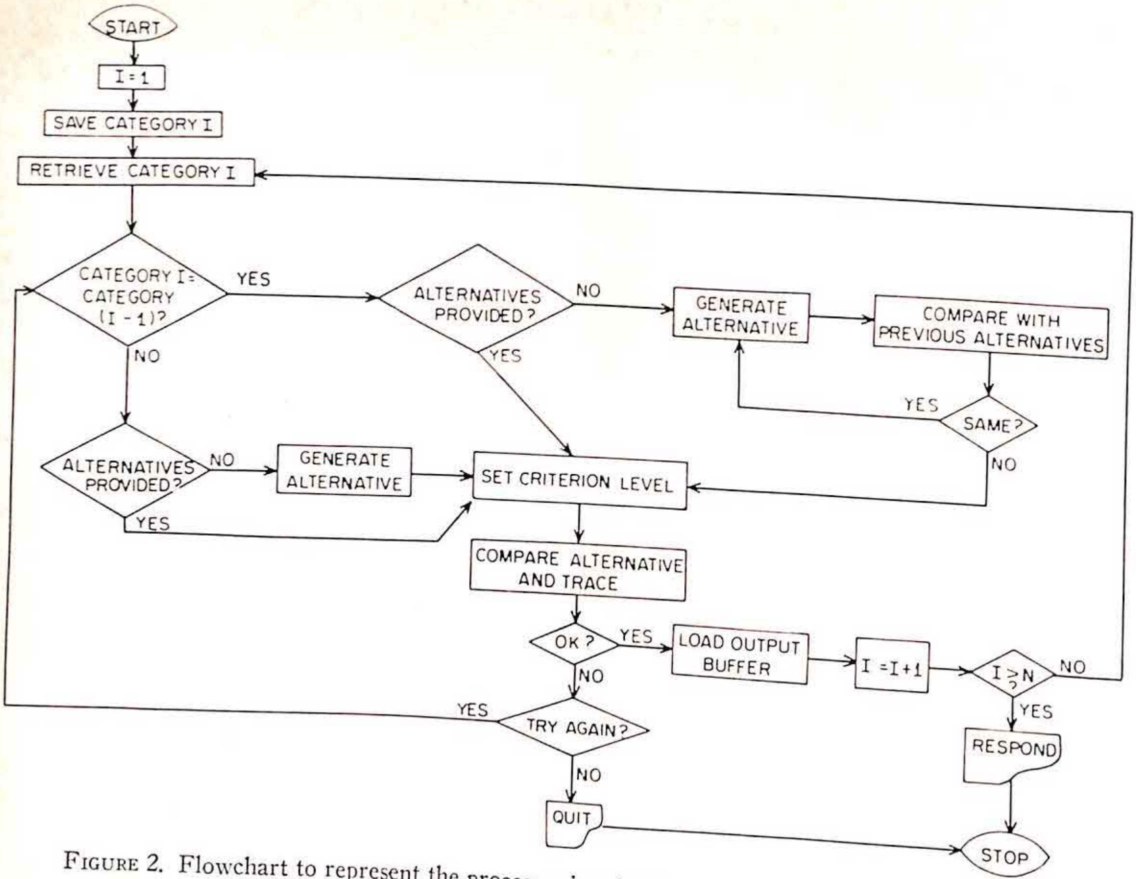

FIGURE 2. Flowchart to represent the processes involved in recall and recognition from same-category or different-category lists.

a bit difficult to know what it can and cannot explain.

A process model could be used to explain these results. One possible process model is shown in Figure 2. This is a flowchart of the processes involved in recall and recognition, and provides a way of explaining the main results shown in Table 7. The general idea is that category retrieval precedes item retrieval (see e.g., Homa, 1973). In the boxes labeled "SAVE CATEGORY I" and "RETRIEVE CATEGORY I," what is meant is the name of the category. For the first item the lower branch is followed; for subsequent loops the branch depends upon whether same-category or different-category trigrams are being retrieved. The recall–recognition distinction is nested in the category branch.

If one compares this flowchart with Figure 3.9 of Murdock (1974) it will be seen that this is a "generate-and-edit" model for recall–recognition differences. Although such a view has been criticized by Tulving and Thomson (1973), a modified view is still defensible (Kintsch, 1974). The locus of the category effect is a comparison loop that applies to same-category trigrams only. The idea is that one is sampling from the category, and it is necessary to monitor the process to ensure sampling without replacement. It would presumably be easy to generate alternatives in the same-category case, but confusion with previous alternatives would lead to greater loss of order information. For recognition, however, one skips directly to the decision part of the system, so recognition of same- and different-category trigrams involve exactly the same process. Recall–recognition differences would be expected by the standard reasoning.

This model has some strengths and weaknesses. On the plus side, it makes explicit some of the processes that must be performed, even if the way we do them is different from the flowchart. It is general enough to represent all the processes involved, and specific enough to pinpoint the locus of the category effect. While the exact operation of the monitoring process is left unspecified, it would not be hard to work out several possibilities.

My own view is that it is not necessarily the best approach. First, I doubt that a generate-and-edit process is really going on here, and introspectively one seems to access the items not the category names. Second, all the effects are assigned to retrieval, and that is surely wrong. There are clearly forgetting effects over time, and they are not represented here at all. Third, while it may provide a convenient way to characterize the overall retrieval processes, it does not seem to illuminate the item-and-order issue at all.

An alternative is to go back to the state diagram of Figure 1 and see how this could be useful in explaining the results. The first point to note is that the general pattern of the results is very consistent with the three-state model suggested. When one is testing item information that means States IO and I are indistinguishable, so the results simply say that the probability of going to State N is greater for different-category trigrams than for same-category trigrams. However, tests of order information discriminate between States IO and I, so the fact that recall differences go the other way is no problem for the model at all.

The obvious extension of the model is as follows. Assume each item in the list can be in State IO, State I, or State N (see Figure 1). Then, what happens at the time of testing depends upon what kind of test is administered. For recall, a trigram will be correct by an ordered-recall criterion if all three items are in State IO or two items are in State IO and the other in State I, correct with probability .5 if one item is in State IO and two in State I, and correct with probability .167 if all three items are in State I. For free recall, the probabilities would be 1.0 in all these cases, since order is not counted in the scoring.

With this approach, the extension to recognition is quite straightforward. Performance will depend not only on the state of each of the three items in the trigram, but also on what the test item is. There are 27 possible state combinations (Items A, B, and C each in State IO, I, and N), and these must be crossed with the six types of test items (ABC, ACB, BAC, BCA, CAB, and CBA). With standard assumptions about guessing it is quite easy to work out ordinal predictions about performance without any knowledge of the values of the transition parameters. Three predictions emerge quite clearly:

1. Recognition will be better than recall.
2. Recognition performance on lures will be more accurate than recognition performance on targets.
3. Recognition performance on remote lures will be better than recognition performance on adjacent lures.

It is the second prediction that is most interesting. The reason basically that it works out this way is that there are more ways a lure can mismatch a partly degraded memory trace than there are ways a target can mismatch a partly degraded memory trace. For instance, suppose the first two items (A and B) are in State I but the third item (C) is in State IO. Then, if the test item is ABC (a target) probability correct will be .5, but if the test item is the lure ACB then probability correct will be 1 because the third item alone is sufficient to identify the probe as new. (The difference between adjacent and remote lures is simply an extension of the above; there are more clear mismatches when all three items of the probe are in the wrong position than when only two of the three are in the wrong position.)

A reexamination of the data of Experiment 4 showed that this prediction is quite wrong. The main data are shown in Table 7, which gives the proportion of correct responses (summed over confidence judgments) for each subject to targets (old items), adjacent lures, and remote lures. With one exception (which resulted from deviant performance on CBA), the third prediction is substantiated; remote lures are more often identified as new than adjacent lures. The problem, however, is that performance on targets is better than on either

TABLE 7

PROPORTION OF CORRECT RESPONSES TO TARGETS, ADJACENT LURES, AND REMOTE LURES FOR SUBJECTS OF EXPERIMENT 4

Subject	Targets (ABC)	Adjacent lures (ACB, BAC)	Remote lures (BCA, CAB, CBA)
1	.907	.662	.813
2	.862	.779	.808
3	.860	.603	.739
4	.967	.945	.910

type of lure, remote or adjacent. This pattern of results holds in all particulars if one does not sum over confidence judgments; that is, if one restricts the analysis to high-confident responses.

This finding poses a real problem for a finite state model of this type. If the recognition process works by an item-by-item comparison of the probe and the memory trace and if each item in the memory trace of the trigram could be in one of several possible states, regardless of whether items are independent or correlated, the lures should be identified more accurately than the targets. One solution to this problem would be to introduce the notion of a response bias and assume that, in cases of doubt, the subject was more likely to give a yes response than a no response. This model does not work very well for item recognition (Vorberg & Schmidt, 1975), and there is no reason to think it is any more appropriate for recognition of order information.

An alternative would be to represent the distinction between item and order information in terms of the nesting model suggested by Murdock (1974). In this model, it is assumed that items are not associated to one another; rather, they are associated to hypothetical coding elements that serve to preserve list structure in memory. A trigram might be coded as

$$** + X_3\{C + X_2[B + X_1(A + *)]\}$$

where the list is the trigram ABC, X_1 to X_3 are the coding elements, and the asterisks are terminators (* denotes the beginning of the list, ** denotes the end of the list). Items are not associated to each other, but to the coding elements, and each pair (an item and a coding element) is nested in the next higher element. (Packing is assumed to occur at the time of list presentation, and unpacking precedes recall. It is assumed that items may be unpacked into an output buffer before the response is actually executed. Details are given in Murdock, 1974, pp. 293–302.)

This model solves one of the classic problems of serial order; namely, why positive transfer in the serial to paired-associate transfer paradigm as predicted by the chaining model fails to occur. Since items are not associated to each other but to coding elements, no positive transfer would be expected unless some mediational process were involved. It also suggests that the stimulus for serial learning is the coding element; item and position probes should be about equally effective. It also predicts a number of phenomena of serial learning and memory, at least at the qualitative level (see pp. 297–300 of Murdock, 1974).

In the present case, one could associate item information with the encoded items (i.e., A, B, and C) and order information with the coding elements (X_1 to X_3). Loss of item information means that the trace of the item is lost, while loss of order information means that the coding element is lost. Item and order information are thus logically separate, and one can lose one without the other. If the list ABCDE is recalled as ABDCE, this would be a case of loss of order (but not item) information; single substitution errors (e.g., ABCXE) would be the reverse. Both types of errors do occur (Conrad, 1959).

When four trigrams are presented as in the MMS method used here, how might this information be represented in memory? For same-category trigrams, the nesting might be as follows:

$$** + W_3\{A_3 + W_2[A_2 + W_1(A_1 + *)]\}$$
$$** + X_3\{B_3 + X_2[B_2 + X_1(B_1 + *)]\}$$
$$** + Y_3\{C_3 + Y_2[C_2 + Y_1(C_1 + *)]\}$$
$$** + Z_3\{D_3 + Z_2[D_2 + Z_1(D_1 + *)]\}$$

where the subscripted As in the first row indicate that all the items in the first trigram are from Category A, the subscripted Bs in the second row indicate that all the items in the second trigram are from Category B, and so on. The coding elements (W, X, Y, and Z) are also subscripted because it is reasonable to suppose that they would have more in common if the list were composed of same-category trigrams than if it were composed of different-category trigrams. Some provision for a coding of the order of the trigrams would also be necessary for a complete picture, but that is not included here.

This model could perhaps explain why targets are more accurately identified than lures (more rapidly too, except for Subject 4). Given a test trigram, the subject compares it with this encoded representation. If it matches, fine; the probe is obviously an old item. If it does not match, then it could be that the memory trace is imperfect or it could be that it should not match because the order of the items really is different. Given this uncertainty there is more room for error, and the decision process might reasonably take longer as well. Admittedly, this is a very free interpretation of the model, but at least it seems to have some intuitive plausibility.

While the coding differences between same-category and different-category trigrams are clear enough, how this is reflected in performance is not. Neither are recall–recognition comparisons particularly illuminated by this nesting model. Consequently, it would be foolish to claim that it really explains the data reported in this article. All that it does is provide a slightly more specific way of conceptualizing the differences between item and order information in memory. However, it is at least as good as any other model we have, and perhaps it may be a useful conceptualization to keep in mind in future research in this area.

Let me conclude by attempting an assessment of what we now know about serial order memory. It is a very general assessment and obviously a personal view, but perhaps it will provide some perspective for research in this area.

1. The distinction between item information and order information is useful and perhaps necessary to understand serial order effects in memory.

2. There are some variables (e.g., time) that affect item and order information the same way, and other variables (e.g., category membership) that affect them differently.

3. There are some situations (e.g., transpositions) where loss of order information precedes loss of item information, but there are also some situations (e.g., single substitution errors) where loss of item information precedes loss of order information.

4. The question of whether item and order information are dependent or independent is probably not the best question to ask. In some situations they may be independent and in others dependent, but this will depend both on the particular situation and the particular measure used to assess them.

5. If loss of order information does occur, it seems to increase the probability of loss of item information.

6. Loss of order information seems to show bowed serial position effects quite clearly. There is at least some evidence that loss of item information does not.

7. It is easier to remember item information in a categorized than a noncategorized list, but the order information depends upon whether recall or recognition is used. For recall, categorized lists are worse whereas, for recognition, there is probably no difference between same-category and different-category lists.

8. How item and order information operate in more general serial tasks such as the buildup and release from proactive inhibition and list-learning studies of serial learning has not yet been studied in any systematic fashion.

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Appendix

TABLE A1

University of Toronto Categorized Word Pool

House parts			
0001: WINDOW	0002: DOOR	0003: ROOF	0004: WALL
0005: FLOOR	0006: CEILING	0007: ROOM	0010: BASEMENT
0011: HEARTH	0012: HALL	0013: CORRIDOR	0014: ELEVATOR
0015: CHIMNEY	0016: FOUNDATION	0017: STEP	0020: STAIRWAY
0021: ATTIC	0022: CUPBOARD	0023: BEDROOM	0024: BATHROOM
0025: CLOSET	0026: DORMER	0027: FOYER	0030: LOBBY
0031: BATHTUB	0032: KITCHEN	0033: DEN	0034: SIDING
0035: CELLAR	0036: ALCOVE	0037: PORCH	0040: LIVINGROOM
Body parts			
0041: LEG	0042: ARMS	0043: HEAD	0044: EYE
0045: FOOT	0046: NOSE	0047: FINGER	0050: EAR
0051: HAND	0052: TOE	0053: MOUTH	0054: STOMACH
0055: HAIR	0056: NECK	0057: HEART	0060: KNEE
0061: CHEST	0062: LIVER	0063: BRAIN	0064: LUNGS
0065: TOOTH	0066: ELBOW	0067: SHOULDER	0070: FACE
0071: TONGUE	0072: ANKLE	0073: THROAT	0074: PELVIS
0075: TRUNK	0076: RIB	0077: INTESTINE	0100: HIP
Ships			
0101: SAILBOAT	0102: DESTROYER	0103: BATTLESHIP	0104: CRUISER
0105: SUBMARINE	0106: ROWBOAT	0107: YACHT	0110: CANOE
0111: FREIGHTER	0112: TUGBOAT	0113: STEAMSHIP	0114: SCHOONER
0115: LINER	0116: MOTORBOAT	0117: CARGOSHIP	0120: CLIPPERSHIP
0121: TANKER	0122: BARGE	0123: SLOOP	0124: OUTRIGGER
0125: SPEEDBOAT	0126: DINGHY	0127: RAFT	0130: RUNABOUT
0131: WARSHIP	0132: FERRY	0133: FRIGATE	0134: HYDROFOIL
0135: INBOARD	0136: SKIFF	0137: CATAMARAN	0140: KAYAK
Birds			
0141: ROBIN	0142: SPARROW	0143: HERON	0144: EAGLE
0145: CROW	0146: CANARY	0147: PARAKEET	0150: HAWK
0151: BLACKBIRD	0152: WREN	0153: ORIOLE	0154: PARROT
0155: PIGEON	0156: STARLING	0157: WOODPECKER	0160: VULTURE
0161: SWALLOW	0162: CHICKEN	0163: DOVE	0164: DUCK
0165: OWL	0166: TITMOUSE	0167: THRUSH	0170: PENGUIN
0171: BLUEJAY	0172: PHEASANT	0173: FINCH	0174: BUZZARD
0175: OSTRICH	0176: FLAMINGO	0177: LARK	0200: PEACOCK
Fruits			
0201: APPLE	0202: TANGELO	0203: PEAR	0204: BANANA
0205: PEACH	0206: GRAPE	0207: CHERRY	0210: PLUM
0211: GRAPEFRUIT	0212: LEMON	0213: TANGERINE	0214: APRICOT
0215: PINEAPPLE	0216: LIME	0217: STRAWBERRY	0220: BLACKBERRY
0221: WATERMELON	0222: PRUNES	0223: CANTALOUPE	0224: RASPBERRY
0225: POMEGRANATE	0226: BLUEBERRY	0227: MANGO	0230: BOYSENBERRY
0231: FIG	0232: RAISIN	0233: NECTARINE	0234: MELON
0235: KUMQUAT	0236: DATE	0237: PAPAYA	0240: HONEYDEW
Tools			
0241: HAMMER	0242: SAW	0243: NAILS	0244: LEVEL
0245: PLANE	0246: CHISEL	0247: RULER	0250: WRENCH
0251: PLIERS	0252: DRILL	0253: SCREWS	0254: PUNCH
0255: SQUARE	0256: FILE	0257: LATHE	0260: SANDPAPER
0261: AWL	0262: SANDER	0263: MALLET	0264: PLUMB
0265: WEDGE	0266: AXE	0267: VISE	0270: CLAMP
0271: NAILSET	0272: CROWBAR	0273: HATCHET	0274: RIPSAW
0275: DRILLPRESS	0276: HACKSAW	0277: RASP	0300: JIGSAW

(table continued)

TABLE A1—(*Continued*)

WEATHER

0301: HURRICANE
0305: HAIL
0311: CYCLONE
0315: TYPHOON
0321: FOG
0325: DROUGHT
0331: CHINOOK
0335: DRIZZLE
0302: TORNADO
0306: SLEET
0312: CLOUDS
0316: THUNDER
0322: MONSOON
0326: WHIRLWIND
0332: MIST
0336: BREEZY
0303: RAIN
0307: STORM
0313: SUNSHINE
0317: SMOG
0323: HUMIDITY
0327: RAINBOW
0333: FROST
0337: SQUALL
0304: SNOW
0310: WIND
0314: LIGHTNING
0320: BLIZZARD
0324: DEW
0330: SNOWSTORM
0334: DUSTSTORM
0340: CLOUDBURST

ELEMENTS

0341: OXYGEN
0345: SODIUM
0351: CHLORINE
0355: URANIUM
0361: PHOSPHORUS
0365: LITHIUM
0371: ANTIMONY
0375: RUBIDIUM
0342: HYDROGEN
0346: KRYPTON
0352: CALCIUM
0356: LEAD
0362: XENON
0366: BROMINE
0372: SILICON
0376: IODINE
0343: NITROGEN
0347: SULPHUR
0353: POTASSIUM
0357: ALUMINUM
0363: ARGON
0367: BORON
0373: NICKEL
0377: COBALT
0344: CARBON
0350: HELIUM
0354: MAGNESIUM
0360: ZINC
0364: BARIUM
0370: TIN
0374: TUNGSTEN
0400: CESIUM

NAMES OF MEN

0401: John
0405: Tom
0411: George
0415: Larry
0421: Dave
0425: Jerry
0431: Elmer
0435: Henry
0402: Bob
0406: Joe
0412: Jack
0416: Frank
0422: Fred
0426: Pete
0432: Bruce
0436: Ron
0403: Bill
0407: Dick
0413: Harry
0417: Paul
0423: Mark
0427: Rick
0433: Gary
0437: Jeff
0404: Jim
0410: Mike
0414: Steve
0420: Sam
0424: Charles
0430: Don
0434: Karl
0440: Ralph

SCIENCES

0441: CHEMISTRY
0445: ZOOLOGY
0451: GEOLOGY
0455: MEDICINE
0461: PHYSIOLOGY
0465: PHILOSOPHY
0471: BACTERIOLOGY
0475: AGRICULTURE
0442: PHYSICS
0446: BOTANY
0452: MICROBIOLOGY
0456: ENGINEERING
0462: ANATOMY
0466: ASTROLOGY
0472: ARCHEOLOGY
0476: OCEANOGRAPHY
0443: PSYCHOLOGY
0447: ASTRONOMY
0453: SOCIOLOGY
0457: BIOCHEMISTRY
0463: ANTHROPOLOGY
0467: GEOGRAPHY
0473: BIOPHYSICS
0477: PATHOLOGY
0444: BIOLOGY
0450: MATHEMATICS
0454: ENTOMOLOGY
0460: METEOROLOGY
0464: GENETICS
0470: METALLURGY
0474: EMBRYOLOGY
0500: AGRONOMY

CITIES

0501: Chicago
0505: Paris
0511: Detroit
0515: Cleveland
0521: Tokyo
0525: Stockholm
0531: Reno
0535: Copenhagen
0502: Helsinki
0506: Miami
0512: Dallas
0516: Pittsburg
0522: Moscow
0526: Jerusalem
0532: Madrid
0536: Naples
0503: Toronto
0507: Boston
0513: Rome
0517: Denver
0523: Atlanta
0527: Cairo
0533: Memphis
0537: Brussels
0504: London
0510: Philadelphia
0514: Bombay
0520: Montreal
0524: Berlin
0530: Vancouver
0534: Shanghai
0540: Venice

MUSICAL INST

0541: PIANO
0545: CLARINET
0551: TROMBONE
0555: CELLO
0561: VIOLA
0565: BASSOON
0571: LUTE
0575: CELESTA
0542: DRUM
0546: FLUTE
0552: OBOE
0556: BANJO
0562: PICCOLO
0566: ACCORDION
0572: HARPSICHORD
0576: TAMBOURINE
0543: TRUMPET
0547: GUITAR
0553: TUBA
0557: KAZOO
0563: CYMBALS
0567: CORNET
0573: BUGLE
0577: BONGOS
0544: VIOLIN
0550: SAXOPHONE
0554: HARP
0560: MARIMBA
0564: HARMONICA
0570: XYLOPHONE
0574: UKULELE
0600: MANDOLIN

TABLE A1—(*Continued*)

	KITCHENWARE		
0601: KNIFE	0602: SPOON	0603: FORK	0604: PAN
0605: POT	0606: SPATULA	0607: STOVE	0610: BOWL
0611: MIXER	0612: CUP	0613: DISH	0614: BEATER
0615: PLATE	0616: SKILLET	0617: REFRIGERATOR	0620: FRYPAN
0621: GLASS	0622: TOASTER	0623: STRAINER	0624: LADLE
0625: OVEN	0626: SAUCER	0627: BLENDER	0630: SIFTER
0631: GRIDDLE	0632: COLANDER	0633: TONGS	0634: BROILER
0635: GRATER	0636: KETTLE	0637: ROLLINGPIN	0640: PERCOLATOR
	AUTOMOBILES		
0641: Ford	0642: Chevrolet	0643: Pontiac	0644: Oldsmobile
0645: Buick	0646: Cadillac	0647: Dodge	0650: Chrysler
0651: Plymouth	0652: Volkswagen	0653: Rambler	0654: Opel
0655: Mercedes	0656: Fiat	0657: Ferrari	0660: Audi
0661: Corvette	0662: Triumph	0663: Vauxhall	0664: Honda
0665: Porsche	0666: Corvair	0667: Mazda	0670: Thunderbird
0671: Saab	0672: Jeep	0673: Simca	0674: Volvo
0675: Datsun	0676: Valiant	0677: Toyota	0700: Renault
	FISH		
0701: Trout	0702: GUPPIE	0703: SHARK	0704: HERRING
0705: PERCH	0706: SALMON	0707: TUNA	0710: GOLDFISH
0711: SUNFISH	0712: COD	0713: BLUEGILL	0714: CARP
0715: PIKE	0716: MINNOW	0717: GUPPY	0720: FLOUNDER
0721: MARLIN	0722: BARRACUDA	0723: BLUEFISH	0724: MACKEREL
0725: DOLPHIN	0726: SHAD	0727: HADDOCK	0730: MUSKIE
0731: CRAPPIE	0732: HALIBUT	0733: SARDINE	0734: EEL
0735: WALLEYE	0736: PIRANHA	0737: STURGEON	0740: GARFISH
	SPORTS		
0741: FOOTBALL	0742: BASEBALL	0743: BASKETBALL	0744: TENNIS
0745: SWIMMING	0746: SOCCER	0747: GOLF	0750: HOCKEY
0751: SNORKLING	0752: LACROSSE	0753: BADMINTON	0754: BOWLING
0755: WRESTLING	0756: ARCHERY	0757: SKIING	0760: FENCING
0761: FISHING	0762: HANDBALL	0763: POLO	0764: BOXING
0765: RUGBY	0766: HUNTING	0767: GYMNASTICS	0770: RACING
0771: SAILING	0772: CURLING	0773: DIVING	0774: JUDO
0775: SURFING	0776: RIDING	0777: CANOEING	1000: SHOOTING
	OCCUPATIONS		
1001: DOCTOR	1002: LAWYER	1003: TEACHER	1004: DENTIST
1005: ENGINEER	1006: PROFESSOR	1007: CARPENTER	1010: SALESMAN
1011: NURSE	1012: PSYCHOLOGIST	1013: PLUMBER	1014: ACCOUNTANT
1015: BUSINESSMAN	1016: SCIENTIST	1017: CLERK	1020: FARMER
1021: UNDERWRITER	1022: POLICEMAN	1023: CHEMIST	1024: MERCHANT
1025: BANKER	1026: PHYSICIST	1027: MUSICIAN	1030: FIREMAN
1031: UNDERTAKER	1032: ELECTRICIAN	1033: JUDGE	1034: MECHANIC
1035: SECRETARY	1036: ARCHITECT	1037: PHARMACIST	1040: WRITER
	TREES		
1041: OAK	1042: MAPLE	1043: PINE	1044: ELM
1045: BIRCH	1046: LINDEN	1047: SPRUCE	1050: REDWOOD
1051: WALNUT	1052: FIR	1053: HICKORY	1054: SYCAMORE
1055: ASH	1056: POPLAR	1057: WILLOW	1060: CHESTNUT
1061: CEDAR	1062: EVERGREEN	1063: SUMAC	1064: BEECH
1065: MAGNOLIA	1066: TEAK	1067: COTTONWOOD	1070: GINKO
1071: SEQUOIA	1072: CYPRESS	1073: EUCALYPTUS	1074: HEMLOCK
1075: SASSAFRAS	1076: ASPEN	1077: BALSAM	1100: COCONUT

(*table continued*)

TABLE A1—(*Continued*)

		CLOTHING	
1101: SHIRT	1102: SOCKS	1103: PANTS	1104: SHOES
1105: BLOUSE	1106: SKIRT	1107: COAT	1110: DRESS
1111: HAT	1112: SWEATER	1113: TIE	1114: SLIP
1115: JACKET	1116: SLACKS	1117: GLOVES	1120: BELT
1121: UNDERWEAR	1122: SHORTS	1123: BRA	1124: SCARF
1125: SUIT	1126: STOCKINGS	1127: TROUSERS	1130: ROBE
1131: VEST	1132: JUMPER	1133: GIRDLE	1134: OVERCOAT
1135: OVERSHOES	1136: RAINCOAT	1137: PANTIES	1140: PAJAMAS
		DISEASES	
1141: CANCER	1142: MEASLES	1143: TUBERCULOSIS	1144: MUMPS
1145: POLIO	1146: CHICKENPOX	1147: MALARIA	1150: LEUKEMIA
1151: PNEUMONIA	1152: SMALLPOX	1153: INFLUENZA	1154: ENCEPHALITIS
1155: LEPROSY	1156: SYPHILLIS	1157: HEPATITIS	1160: TYPHOID
1161: RICKETS	1162: DIABETES	1163: DIPHTHERIA	1164: BERIBERI
1165: ARTHRITIS	1166: SCURVY	1167: EPILEPSY	1170: EMPHYSEMA
1171: CHOLERA	1172: HEMOPHELIA	1173: TETANUS	1174: JAUNDICE
1175: BRONCHITIS	1176: RABIES	1177: DYSENTERY	1200: MENINGITIS
		TOPOGRAPHY	
1201: MOUNTAIN	1202: HILL	1203: VALLEY	1204: RIVER
1205: ROCK	1206: LAKE	1207: CANYON	1210: TUNDRA
1211: OCEAN	1212: CAVE	1213: PLAIN	1214: VOLCANO
1215: PLATEAU	1216: GULLY	1217: STREAM	1220: SEA
1221: DESERT	1222: CREVICE	1223: ISLAND	1224: GLACIER
1225: ISTHMUS	1226: RAVINE	1227: RIDGE	1230: CRATER
1231: MUSKEG	1232: PENINSULA	1233: COVE	1234: DELTA
1235: BAY	1236: DRUMLIN	1237: GEYSER	1240: MORAINE
		CRIMES	
1241: MURDER	1242: RAPE	1243: ROBBERY	1244: THEFT
1245: ASSAULT	1246: ARSON	1247: KIDNAPPING	1250: LARCENY
1251: EMBEZZLING	1252: ADULTERY	1253: BATTERY	1254: FORGERY
1255: TREASON	1256: FRAUD	1257: SUICIDE	1260: PERJURY
1261: MANSLAUGHTER	1262: BLACKMAIL	1263: BIGAMY	1264: EXTORTION
1265: INCEST	1266: BRIBERY	1267: SLANDER	1270: VANDALISM
1271: GAMBLING	1272: LIBEL	1273: SODOMY	1274: ESPIONAGE
1275: SABOTAGE	1276: VAGRANCY	1277: SMUGGLING	1300: SWINDLING
		COUNTRIES	
1301: France	1302: Russia	1303: England	1304: Germany
1305: Canada	1306: Italy	1307: Spain	1310: Mexico
1311: Ethiopia	1312: Japan	1313: Sweden	1314: Brazil
1315: Switzerland	1316: Ireland	1317: Australia	1320: Norway
1321: India	1322: Argentina	1323: Denmark	1324: Greece
1325: Poland	1326: Israel	1327: Austria	1330: Scotland
1331: Belgium	1332: Cuba	1333: Portugal	1334: Egypt
1335: Finland	1336: Pakistan	1337: Venezuela	1340: Iran
		FLOWERS	
1341: ROSE	1342: TULIP	1343: CARNATION	1344: DAISY
1345: DAHLIA	1346: ORCHID	1347: LILY	1350: PANSY
1351: PETUNIA	1352: GARDENIA	1353: DAFFODIL	1354: DANDELION
1355: NARCISSUS	1356: LILAC	1357: GERANIUM	1360: PEONY
1361: SUNFLOWER	1362: AZALIA	1363: GLADIOLA	1364: POPPY
1365: ZINNIA	1366: JONQUIL	1367: BUTTERCUP	1370: MARIGOLD
1371: MAGNOLIA	1372: ASTER	1373: HYACINTH	1374: CAMELLIA
1375: BEGONIA	1376: POINSETTIA	1377: CROCUS	1400: FORSYTHIA

TABLE A1—(*Continued*)

Animals			
1401: DOG	1402: CAT	1403: HORSE	1404: COW
1405: LION	1406: TIGER	1407: ELEPHANT	1410: PIG
1411: BEAR	1412: MOUSE	1413: RAT	1414: DEER
1415: SHEEP	1416: GIRAFFE	1417: GOAT	1420: ZEBRA
1421: SQUIRREL	1422: WOLF	1423: DONKEY	1424: RABBIT
1425: LEOPARD	1426: MULE	1427: FOX	1430: MOOSE
1431: RHINOCEROS	1432: CAMEL	1433: ANTELOPE	1434: HIPPOPOTAMUS
1435: MONKEY	1436: RACOON	1437: PANTHER	1440: LLAMA
Insects			
1441: FLY	1442: ANT	1443: BEE	1444: MOSQUITO
1445: SPIDER	1446: BEETLE	1447: MILLIPEDE	1450: WASP
1451: GRASSHOPPER	1452: GNAT	1453: MOTH	1454: FLEA
1455: BUTTERFLY	1456: COCKROACH	1457: TERMITE	1460: HORNET
1461: MITE	1462: CATERPILLAR	1463: TICK	1464: WATERBUG
1465: CENTIPEDE	1466: LOUSE	1467: HORSEFLY	1470: APHID
1471: CICADA	1472: TARANTULA	1473: SCORPION	1474: KATYDID
1475: WEEVIL	1476: FIREFLY	1477: SLUG	1500: CUTWORM
Colors			
1501: BLUE	1502: RED	1503: GREEN	1504: YELLOW
1505: BLACK	1506: PURPLE	1507: WHITE	1510: PINK
1511: BROWN	1512: BLONDE	1513: GRAY	1514: TURQUOISE
1515: INDIGO	1516: MAROON	1517: CHARTREUSE	1520: TAN
1521: LAVENDER	1522: BEIGE	1523: AQUA	1524: MAGENTA
1525: SCARLET	1526: MAUVE	1527: RUSSET	1530: BURGUNDY
1531: FUCHSIA	1532: AZURE	1533: CRIMSON	1534: AMBER
1535: AUBURN	1536: EBONY	1537: IVORY	1540: TAUPE
U.S. states			
1541: Ohio	1542: California	1543: Pennsylvania	1544: Florida
1545: Michigan	1546: Indiana	1547: Illinois	1550: Texas
1551: Maryland	1552: Virginia	1553: Maine	1554: Kentucky
1555: Connecticut	1556: Alaska	1557: Arkansas	1560: Vermont
1561: Colorado	1562: Delaware	1563: Oregon	1564: Montana
1565: Hawaii	1566: Wyoming	1567: Utah	1570: Arizona
1571: Iowa	1572: Oklahoma	1573: Missouri	1574: Kansas
1575: Wisconsin	1576: Alabama	1577: Nebraska	1600: Georgia
Textiles			
1601: COTTON	1602: WOOL	1603: SILK	1604: RAYON
1605: DACRON	1606: LINEN	1607: SATIN	1610: ORLON
1611: MADRAS	1612: VELVET	1613: BURLAP	1614: DENIM
1615: CORDUROY	1616: FLANNEL	1617: MOHAIR	1620: MUSLIN
1621: TWEED	1622: SEERSUCKER	1623: CANVAS	1624: ACRILON
1625: CHIFFON	1626: CREPE	1627: FELT	1630: GABARDINE
1631: LACE	1632: GINGHAM	1633: ARNEL	1634: SUEDE
1635: CASHMERE	1636: POLYESTER	1637: BANLON	1640: CELANESE
Seasonings			
1641: SALT	1642: PEPPER	1643: SUGAR	1644: GARLIC
1645: CARDAMON	1646: CINNAMON	1647: CLOVES	1650: PAPRIKA
1651: OREGANO	1652: KETCHUP	1653: MUSTARD	1654: SAVOURY
1655: NUTMEG	1656: VINEGAR	1657: CARAWAY	1660: THYME
1661: PARSLEY	1662: ALMOND	1663: SAGE	1664: CHIVES
1665: GINGER	1666: MINT	1667: DILL	1670: BASIL
1671: CURRY	1672: ANISE	1673: CUMIN	1674: MARJORAM
1675: PEPPERMINT	1676: SESAME	1677: CHILI	1700: ALLSPICE

(*table continued*)

TABLE A1—*(Continued)*

	VEGETABLES		
1701:CARROT	1702:PEA	1703:BEAN	1704:CORN
1705:ZUCCHINI	1706:POTATO	1707:LETTUCE	1710:SPINACH
1711:BEET	1712:BROCCOLI	1713:CELERY	1714:ASPARAGUS
1715:YAMS	1716:TURNIP	1717:GREENBEANS	1720:CABBAGE
1721:CAULIFLOWER	1722:WATERCRESS	1723:LIMABEAN	1724:RADISH
1725:STRINGBEAN	1726:CUCUMBER	1727:ARTICHOKE	1730:RUTABAGA
1731:RHUBARB	1732:WAXBEANS	1733:CHARD	1734:EGGPLANT
1735:PUMPKIN	1736:KOLRABI	1737:ONION	1740:OKRA
	CLERGY		
1741:PRIEST	1742:MINISTER	1743:BISHOP	1744:RABBI
1745:POPE	1746:PASTOR	1747:DEACON	1750:REVEREND
1751:NUN	1752:PREACHER	1753:ARCHBISHOP	1754:ELDER
1755:PARSON	1756:MISSIONARY	1757:SISTER	1760:MONSIGNOR
1761:CURATE	1762:FRIAR	1763:VICAR	1764:RECTOR
1765:PADRE	1766:CHAPLAIN	1767:LAYMAN	1770:ABBOT
1771:EVANGELIST	1772:BROTHER	1773:CANTOR	1774:SEXTON
1775:CLERIC	1776:MONK	1777:FATHER	2000:CANNON

(Received October 16, 1975)

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Editorial

After a year's experience with the editing of *Journal of Experimental Psychology: General* it is clear that my original editorial policies (Kimble, 1975) need some revision. This editorial announces several changes based on some of my successes and failures in the first year.

Number of Manuscripts Submitted

This has been my biggest problem. The "new editor effect" had a short life in the case of JEP:G and, after an initial flurry of inappropriate articles, my submission rate sank to a trickle. Fortunately the quality of these later manuscripts was high and most of them were published. Things are picking up now, but I still operate with far too little material. As I did in 1975, I encourage you to submit integrative articles in any area of experimental psychology.

Editorial Review

The concept of inviting authors to nominate reviewers was my most conspicuous success. Authors showed excellent taste in the recommendations they made, and the quality of the reviews I received was beyond my highest expectations. There was no evidence at all to support the claim of an "old boy" (or "old girl") phenomenon. In fact, it quickly became clear that author-nominated reviewers would pull no punches in spite of the fact that they had no protective anonymity. The readers have strongly endorsed the policy of publishing some of these reviews. Beginning this year I will also publish occasional commentaries on earlier articles. One or two examples will appear in early issues of the Journal.

The practices I have followed in obtaining reviews are cumbersome in several ways and sometimes they slow up the editorial process. What is worse is, that with so few manuscripts, I have sometimes had to cut things short in order to meet deadlines. Even then two issues of the Journal were smaller in 1975 than they should have been. In an effort to improve this aspect of the situation I have secured the cooperation of a continuing board of consulting editors whose names appear on the cover of this issue of the Journal.

Criteria of Appropriateness

The features of the articles I am looking for remain (1) integrative equality and (2) appeal to a range of readers. The first of these criteria means that manuscripts submitted to JEP: G are usually long. Experience reveals, however, that there are times when a shorter article is appropriate: (1) when it is truly integrative and (2) when it relates directly to an earlier article in the Journal. I encourage authors to submit manuscripts that meet either of these criteria.

Summaries

Finally a small point related to format: The summaries that introduce articles in JEP: G have created two minor problems. These are (1) a few readers have suggested that a shorter summary than most of those published in 1975 would be more useful, and (2) the longer summaries create awkward problems in production when they extend beyond the first page of the published article. Because the solutions to these two problems are the same, I am now requesting that summaries be limited to about 450 words.

REFERENCE

Kimble, G. A. Editorial. *Journal of Experimental Psychology: General,* 1975, *104,* 1–4.

Gregory A. Kimble
Editor

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Learned Helplessness: Theory and Evidence

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SUMMARY

In 1967, Overmier and Seligman found that dogs exposed to inescapable and unavoidable electric shocks in one situation later failed to learn to escape shock in a different situation where escape was possible. Shortly thereafter Seligman and Maier (1967) demonstrated that this effect was caused by the uncontrollability of the original shocks. In this article we review the effects of exposing organisms to aversive events which they cannot control, and we review the explanations which have been offered.

There seem to be motivational, cognitive, and emotional effects of uncontrollability. (a) Motivation. Dogs that have been exposed to inescapable shocks do not subsequently initiate escape response in the presence of shock. We review parallel phenomena in cats, fish, rats, and man. Of particular interest is the discussion of learned helplessness in rats and man. Rats are of interest because learned helplessness has been difficult to demonstrate in rats. However, we show that inescapably shocked rats do fail to learn to escape if the escape task is reasonably difficult. With regard to man, we review a variety of studies using inescapable noise and unsolvable problems as agents which produce learned helplessness effects on both instrumental and cognitive tasks. (b) Cognition. We argue that exposure to uncontrollable events interferes with the organism's tendency to perceive contingent relationships between its behavior and outcomes. Here we review a variety of studies showing such a cognitive set. (c) Emotion. We review a variety of experiments which show that uncontrollable aversive events produce greater emotional disruption than do controllable aversive events.

We have proposed an explanation for these effects, which we call the learned helplessness hypothesis. It argues that when events are uncontrollable the organism learns that its behavior and outcomes are independent, and that this learning produces the motivational, cognitive, and emotional effects of uncontrollability. We describe the learned helplessness hypothesis and research which supports it.

Finally, we describe and discuss in detail alternative hypotheses which have been offered as accounts of the learned helplessness effect. One set of hypotheses argues that organisms learn motor responses during exposure to uncontrollable shock that compete with the response required in the test task. Another explanation holds that uncontrollable shock is a severe stressor and depletes a neurochemical necessary for the mediation of movement. We examine the logical structure of these explanations and present a variety of evidence which bears on them directly.

What follows are three instances of the phenomenon to be explained:

1. When placed in a shuttle box an experimentally naive dog, at the onset of the first electric shock, runs frantically about, until it accidentally scrambles over the barrier and escapes the shock. On the next trial, the dog, running frantically, crosses the barrier more quickly than on the preceding trial. Within a few trials the animal becomes very efficient at escaping and soon learns to avoid shock altogether. After about 50 trials the dog becomes nonchalant and stands in front of the barrier. At the onset of the signal for shock, the dog leaps gracefully across and rarely gets shocked again. But dogs first given inescapable shock in a Pavlovian hammock show a strikingly different pattern. Such a dog's first reactions to shock in the shuttle box are much the same as those of a naive dog. He runs around frantically for about 30 sec, but then stops moving, lies down, and quietly whines. After 1 min. of this, shock terminates automatically. The dog fails to cross the barrier and escape from shock. On the next trial, the dog again fails to escape. At first he struggles a bit and then, after a few seconds, seems to give up and passively accept the shock. On all succeeding trials, the dog continues to fail to escape.

2. A college student is confronted with a series of 25 letter anagrams, each with the same pattern, 34251. He has a little trouble with the first one, taking about 45 sec to solve it. He solves each of the next three in about 30 sec, and now he sees the pattern. Each of the last 16 anagrams is solved immediately. In striking contrast is the college student who has first faced a series of discrimination problems which are unsolvable or a series of loud tones which are inescapable. He works hard on the first anagram, trying many rearrangements of letters, but fails to solve it in the 100 sec allowed. He fails to solve the second one also. The third anagram, a relatively easy one, he solves, after about 60 sec. He fails to solve the next eight, appearing to give up after about 60 sec with each one. He then solves six in a row, but very slowly, and finally sees the pattern. He now solves the final three immediately.

3. A naive rat is placed in a shuttle box and trained to escape from shock. Shock terminates immediately upon his running to the opposite side; he learns readily, and escapes efficiently. A second rat who had received inescapable shock earlier in another apparatus learns just as well as the first rat to escape in the shuttle box. Now, however, the contingency between crossing the shuttle box and shock termination is obfuscated; shock does not terminate immediately upon crossing, but only after 3 sec elapse. The first rat continues to escape readily, learning to bridge a 3-sec delay of reinforcement. The second rat, however, fails to respond; on other trials, he runs across during shock, but overall he shows no learning curve.

We believe these three phenomena are all instances of "learned helplessness," instances in which an organism has learned that outcomes are uncontrollable by his responses and is seriously debilitated by this knowledge. This article explores the evidence for the phenomenon of learned helplessness, and discusses a variety of theoretical interpretations. Since the phenomenon results from experience with uncontrollable outcomes, we begin by defining uncontrollability.

UNCONTROLLABILITY AND THE INSTRUMENTAL TRAINING SPACE

Learning theorists have usually viewed the relations between instrumental responding and outcomes to which organisms are sensitive in terms of the conditional probability of an outcome or reinforcer following a response $p(RF/R)$, which can have values ranging from 0 to 1.0. At 1.0, every response produces a reinforcer or outcome (continuous reinforcement). At 0, a response never produces a reinforcer (extinction). Intermediate values represent various degrees of partial reinforcement.

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One conditional probability, however, is an inadequate description of the relations between response and outcomes about which an organism may learn. Important events can sometimes occur when no specific response has been made, and it would be a woefully maladaptive organism that was insensitive to such a contingency. Rather than representing environmental contingencies as occurring along a single dimension, we think instrumental training can be better described using a two-dimensional space, as shown in Figure 1. The x-axis $p(\mathrm{RF}/\mathrm{R})$ represents the traditional dimension, the conditional probability of an outcome following a response. Orthogonal to the conditional probability of an outcome, given a response, is the conditional probability of an outcome occurring in the absence of that response $p(\mathrm{RF}/\overline{\mathrm{R}})$. This dimension is represented along the y-axis. We assume that organisms are sensitive to variations along *both* dimensions conjointly, and the empirical meaning of this assumption is that systematic changes in behavior should occur with systematic changes along both dimensions. There is considerable convergence of opinion and evidence among learning theorists today that organisms can indeed learn about the contingencies within this instrumental training space, including the crucial 45° line (e.g., Catania, 1971; Church, 1969;; Gibbon, Berryman, & Thompson, 1974; Maier, Seligman, & Solomon, 1969; Rescorla, 1967, 1968; Seligman, Maier, & Solomon, 1971; Wagner, 1969; Weiss, 1968). Thus an organism may learn the extent to which food occurs when it does *not* make a specific response along with learning the extent to which food occurs when it *does* make a specific response.

Consider a few examples. In Figure 1, Point a (1.0,0) is a case of continuous reinforcement: The subject is always reinforced for response R, and is never reinforced if it fails to make R. Point b (0,1.0) is a case in which the subject is never reinforced for making the designated R, and is always reinforced for refraining from R (differential reinforcement of other behavior). Consider Point c (.5,.2): Here the subject is reinforced 50% of the times that it makes R, but even if it fails to make R, it is reinforced 20% of the time.

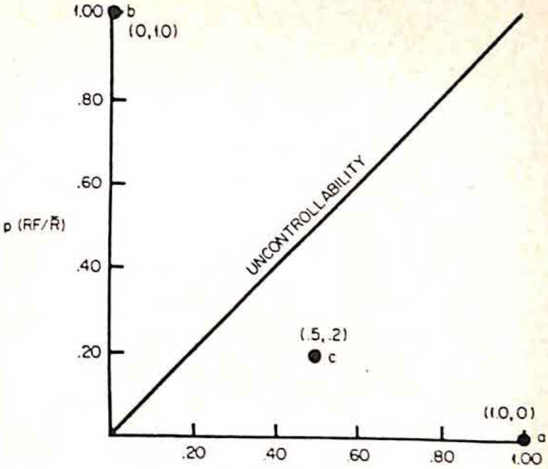

FIGURE 1. The response-reinforced contingency space. $p(\mathrm{RF}/\mathrm{R})$ = conditional probability of an outcome following a response, $p(\mathrm{RF}/\overline{\mathrm{R}})$ = conditional probability of an outcome occurring in the absence of that response.

The traditional training procedures arrayed along the x-axis have been thoroughly explored by many experimenters (e.g., Ferster & Skinner, 1957; Honig, 1966). The points in the training space which do not fall along the x-axis have not, however, been systematically investigated. Consider the points that lie along the 45° line (x, y, where $x = y$). Whether or not the subject responds, the density of reinforcement is the same. The conditional probability of an outcome, given a specific response, *does not differ* from the conditional probability of reinforcement in the absence of that response. The outcome is independent of responding.

The concepts of controllability and uncontrollability are defined within this instrumental training space. Any time there is something the subject can do or refrain from doing that changes what it gets, it has control. Specifically, a response R stands in a relation of *control* to a reinforcer RF if and only if

$$p(\mathrm{RF}/\mathrm{R}) \neq p(\mathrm{RF}/\overline{\mathrm{R}}). \qquad (1)$$

Furthermore, when a response will not change what the subject gets, the response and reinforcer are independent. Specifically, a response R stands in relation of independence to a reinforcer RF if and only if

$$p(\mathrm{RF}/\mathrm{R}) = p(\mathrm{RF}/\bar{\mathrm{R}}). \tag{2}$$

When this is true of all emitted responses (as in Pavlovian conditioning) the subject *cannot control* the reinforcer, and the reinforcer is defined as *uncontrollable.*

How can we tell that the phenomena we will discuss result from experiencing uncontrollable outcomes as opposed to merely experiencing the outcome itself? To put it another way, how can we tell whether helplessness is a psychological phenomenon as opposed to merely being the result of physical stimulation?

There is a simple and elegant experimental design which isolates the effects of controllability from the effects of the outcome being controlled. In this "triadic" design, three groups are used: One group receives as its pretreatment an outcome that it can control by some response. A second group is "yoked"—it receives *exactly* the same physical outcome as its counterpart in the first group, but there is no response the yoked subject can make which modifies these outcomes. A third group receives no pretreatment. Later, all groups are tested on a new task.

Helplessness does not result from trauma per se: In the studies we discuss, deficits do not occur in the groups that control shock, but only in the yoked group (Hiroto & Seligman, 1975; Maier, 1970; Maier, Anderson, & Lieberman, 1972; Seligman & Beagley, 1975; Seligman & Maier, 1967).[1] The triadic design is a direct test of the hypothesis that learning that shock is uncontrollable, and not shock per se, causes helplessness. Here is an example of how the triadic design is used: Seligman and Maier (1967) used three groups of eight dogs. An escape group was trained in a hammock to turn off shock by pressing a panel with its nose. A yoked group received shocks identical in number, duration, and pattern to the shocks delivered to the escape group. The yoked group differed from the escape group only with respect to the instrumental control which it had over shock; while pressing the panel did not affect the programmed shocks in the yoked group, panel pressing terminated shock in the escape group. A naive control group received no shock in the hammock.

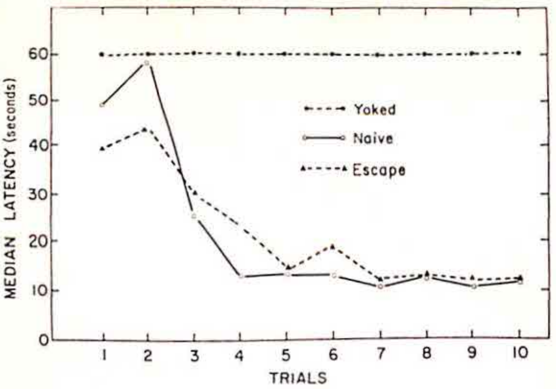

FIGURE 2. Median response latency in a shuttle box for dogs given escapable, yoked inescapable, or no shock in a harness. (The yoked group did not learn to escape.) (From "Failure to Escape Traumatic Shock" by Martin E. P. Seligman and Steven F. Maier, *Journal of Experimental Psychology,* 1967, *74,* 1–9. Copyright 1967 by the American Psychological Association. Reprinted by permission.)

Twenty-four hours after the hammock treatment, all three groups received escape/avoidance training in a shuttle box. Figure 2 shows the results of this experiment. The escape group and the naive control group performed well in the shuttle box. They jumped the barrier readily. In contrast, the yoked group was significantly slower to respond than the escape group and the naive control group. Six of the eight subjects in the yoked group failed completely to escape shock. So it was *not the shock itself,* but the inability to control the shock, that produced failure to respond.

EFFECTS OF UNCONTROLLABILITY

Having defined the objective conditions under which uncontrollability occurs and delineated the kind of experimental design which isolates the effects of uncontrollability from the effects of stimulation per se, we now review the deficits produced by uncontrollable outcomes. In general, when an organism experiences uncontrollable events, three deficits often ensue: motivational, cognitive, and emotional.

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[1] It should be mentioned that Church (1963) has argued against the use of the yoked group as a *control* group for instrumental learning. This argument is not relevant to helplessness experiments in which the yoked group is the experimental group, and the other groups are each control groups.

a) The motivation to respond in the face of later aversive events seems to wane. b) Moreover, even if the subject does respond and the response succeeds in producing relief, the subject often has difficulty learning that the response worked. c) Finally, emotional balance may be distributed; depression and anxiety, measured in a variety of ways, may predominate. The motivational deficits produced by helplessness are in many ways the most striking, so we turn to them first for review and analysis.

Motivational Deficits

Dogs. These sets of experiments form the base of the pyramid on which we construct a theory of learned helplessness, so these studies will be examined thoroughly. The behavior of dogs exposed to inescapable shock seems to typify what many species do when they are faced with uncontrollability.

Here is the typical procedure that produces learned helplessness in the dog (Overmier, 1968; Overmier & Seligman, 1967; Seligman & Groves, 1970; Seligman & Maier, 1967; Seligman, Maier, & Geer, 1968). On the first day, the subject is strapped into a hammock and given 64 inescapable electric shocks, each 5.0 sec long and of 6.0 mA (moderately painful) intensity. The shocks are not predicted by any signal and they occur randomly in time. Twenty-four hours later, the subject is given 10 trials of signalized escape/avoidance training in a two-way shuttle box. The dog must jump over the barrier from one compartment into the other to escape or avoid shock. Shocks can occur in either compartment, so there is no *place* that is always safe, but the *response* of shuttling or jumping always leads to shock termination.

The onset of a signal (light dimming) begins each trial, and the signal stays on until the trial ends. The interval between the start of the signal and the shock is 10 sec. If the dog jumps the shoulder-high barrier during this interval, the signal terminates and shock is prevented. Failure to jump during the signal-shock interval leads to a 4.5-mA shock which remains until the dog jumps the barrier. If the dog fails to jump the barrier within 60 sec after signal onset, the trial automatically ends.

Between the years 1965 and 1969 the behavior of about 150 dogs that received prior inescapable shock was studied. Of these, two thirds (about 100) did not learn to escape and went through the striking sequence of behaviors that we described. The other one third seemed completely normal. Like naive dogs, they escaped efficiently. There was no intermediate outcome. Interestingly enough, of the several hundred naive dogs who had been given shuttle box training, about 95% have been efficient responders. The other 5% failed to learn even with no prior inescapable shock. We believe that the prior history of these dogs before they arrived at the laboratory may determine whether a naive dog will show a learned helplessness effect and whether a dog given inescapable shock will be immune to this effect. When we discuss the theory and prevention of the learned helplessness effect below, we will be more explicit about how to immunize against failure to escape.

Since dogs exposed to inescapable shock seem to be physically capable of jumping the barrier, the behavioral deficit must have a psychological base. Occasionally, they jump the barrier between trials. Further, if a dog has been sitting and taking shock after shock on the left side of the box, and the door on the right side is opened at the end of the session, he will often come bounding across to escape from the box altogether

The learned helplessness effect in the dog occurs in a variety of situations and is easily produced. Within limits, it does not depend on the use of any particular shock parameters. We have varied the frequency, intensity, duration, and temporal pattern of shocks, and still produced the effect. Furthermore, it does not matter if the inescapable shock is or is not preceded by a signal. Finally, it does not matter what apparatus the inescapable shocks are given in or where the escape/avoidance training takes place. The shuttle box and hammock are interchangeable. If the dog is given inescapable shock in the shuttle box and then required to press panels with its head to escape in the hammock, he is still helpless. Further, after uncontrollable shock, dogs are not only debilitated at fleeing from the shock itself (escape), but they also seem to be unable to prevent or "avoid" shock. Overmier (1968) gave dogs inescapable shock in the hammock and then tested them in a shuttle box. If the dog jumped after the signal went on, but before the shock came on, he could avoid the shock. But no escape was allowed, for if the dog failed to jump in the signal-shock interval, the barrier was closed and inescapable shock occurred. The helpless dogs failed to avoid, just as they had failed to escape. So, dogs previously exposed to inescapable shocks failed to cope adaptively with signals for shock as well as with shock.

Debilitation of response initiation as a consequence of uncontrollable outcomes has been reported in cats, rats, mice, birds, primates, fish, and man, as well as in dogs. The learned helplessness effect seems rather general among species that learn.

Cats. Thomas and Balter (in press) reported an effect in cats which seems identical to learned helplessness in dogs. (See also Masserman, 1943, 1971; Seward & Humphrey, 1967; Zielinski & Soltysik, 1964, for other reports of debilitation in cats caused by inescapable shock). They designed a hammock for cats and gave them inescapable shock. When later placed in a cat shuttle box, those cats failed to escape. Like dogs, they sat and took the shock. Thomas and Balter also reported that blocking activity of the septal area of the brain breaks up helplessness, and directly stimulating the septum electrically produces a learned helplessness-like effect. When we discuss the theory and therapy for helplessness, we will examine these physiological data in more detail.

Fish. Following inescapable shock, fish also show poor escape and avoidance responding. Padilla, Padilla, Ketterer, and Giacolone (1970) gave inescapable shock to goldfish and then tested them in an aquatic shuttle box. These fish were slower to avoid than naive controls. (See Behrend & Bitterman, 1963; Bintz, 1971; Frumkin & Brookshire, 1969; Padilla, 1973, for related goldfish data.)

Rats. Until recently, it has proven difficult to produce a learned helplessness effect in rats. A substantial number of experiments were performed involving inescapable shock; by and large, however, these showed rather small, if any, effects on later response initiation. (Maier et al., 1969, and Seligman et al., 1971, reviewed the complicated literature, and the interested reader is referred there for details. See also Anderson, Cole, & McVaugh, 1968; de Toledo & Black, 1967; Dinsmoor & Campbell, 1965a, 1965b; Looney & Cohen, 1972; Mullin & Mogenson, 1963; Weiss, Krieckhaus, & Conte, 1968, for representative experimental studies.) Unlike dogs, a rat given prior inescapable shock was typically only a bit slower to escape shock on the first few trials, or slower to acquire avoidance—it did not fail to learn.

After intensive experimentation, however, several investigators have now independently produced substantial learned helplessness effects in rats. (Maier, Albin, & Testa, 1973; Maier & Testa, 1975; Seligman & Beagley, 1975; Seligman, Rosellini, & Kozak, 1975.)[2] In doing so, one crucial factor emerged—the response used in the test for learned helplessness must be difficult, and not something the rat does very readily. So, for example, if rats are first exposed to inescapable shock and then tested on a simple escape response like pressing a bar *once* (FR-1), or fleeing to the other side of a shuttle box, no deficits are found. If, however, the response requirement is increased—so the bar must be pressed three times in order for shock to end (FR-3 lever pressing) or the rat has to run across the shuttle box and back (FR-2 shuttling)—then the rat that has experienced inescapable shock fails to escape. In con-

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[2] In passing it should be mentioned that mice have been reported to show response deficits following inescapable shock (Braud, Wepman, & Russo, 1969).

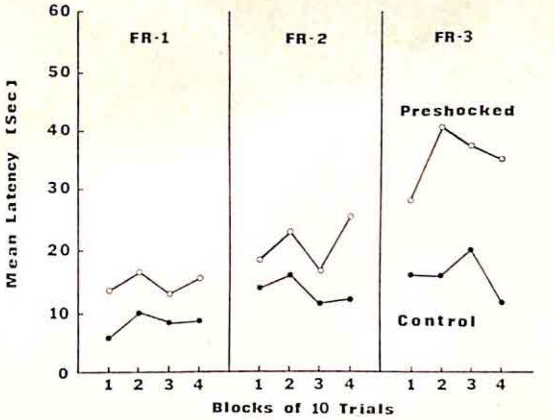

FIGURE 3. Mean latency to press a lever to escape shock in an FR-1, FR-2, and FR-3 schedule, for rats given inescapable or no shocks. (The yoked subjects escaped very poorly only on the FR-3.) (From "Learned Helplessness in the Rat" by Martin E. P. Seligman and Gwyneth Beagley, *Journal of Comparative and Physiological Psychology*, 1975, *88*, 534–541. Copyright 1975 by the American Psychological Association. Reprinted by permission.)

trast, rats that have had prior escapable shock or no shock learn even more difficult responses without difficulty (see Figure 3).

Hannum, Rosellini, & Seligman (in press) recently extended these findings to development. Three groups of rats received four sessions of escapable, inescapable, or no shock shortly after they were weaned. At 90 days of age they were tested on an FR-3 lever press escape task. Rats that had received inescapable shock at weaning failed to escape shock. Rats that had received escapable shock or no shock escaped well (see Figure 4).

Man. What are the laboratory effects of inescapable trauma in man? Like the dog, cat, rat, and fish, when a human being is faced with noxious events that it cannot control, its motivation to respond seems to be reduced.

Hiroto's study (1974) is representative and it replicated in college students the findings on dogs. In a triadic design, subjects in his escape group received loud noise which they learned to turn off by button pushing. The subjects in the inescapable group received the same noise, but the noise was independent of their responding. A third group received no noise. All the groups were then taken to a hand shuttle box; in order to escape noise the individual had only to move his hand from one side to the other. Both the no-noise and escape group learned readily to shuttle with their hands. Like other species, however, the human inescapable group failed to escape and avoid; rather, most sat passively and took the aversive noise. (For other learned helplessness experiments in man, see Fosco & Geer, 1971; Glass & Singer, 1972; Hiroto & Seligman, 1975; Klein, Fencil-Morse, & Seligman, in press, Krantz, Glass, & Snyder, 1974; Miller & Seligman, 1975a, Note 1; Racinskas, 1971; Rodin, 1975, Roth, 1973; Roth & Bootzin, 1974; Roth & Kubal 1975; Thornton & Jacobs, 1971.)

Hiroto's design was actually more complicated and had two other important factors. Half the subjects in each of the three groups were told that how they did in the shuttle box was a test of skill; the other half were told that how they did was governed by chance. Those who received chance instructions tended to respond poorly in all groups. Finally, the personality dimension of "external vs. internal locus of control of reinforcement" was also varied in Hiroto's design (see Lefcourt, 1966; Rotter, 1966; James, Note 2), with half of all the students in each group being "externals" and half

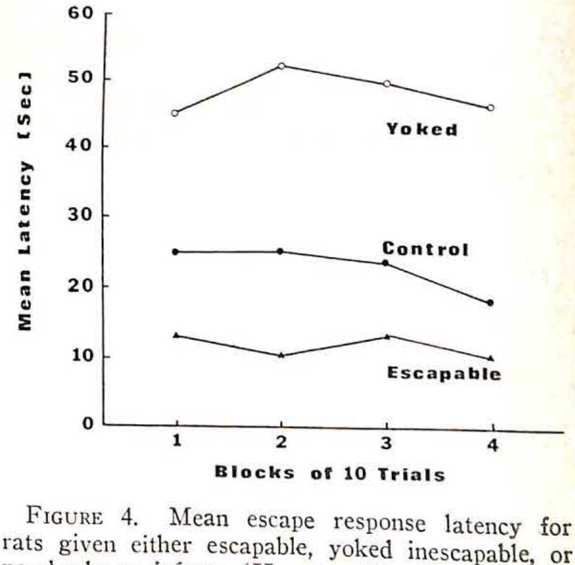

FIGURE 4. Mean escape response latency for rats given either escapable, yoked inescapable, or no shocks as infants (Hannum, Rosellini, & Seligman, in press). The rats given inescapable shocks as infants did not learn to escape.)

"internals." An external is a person who believes, as shown by his answers on a personality inventory, that reinforcements occur in his life by chance or luck and are beyond his control. An internal believes that *he* controls his own reinforcers and that skill will out. Hiroto found that externals became helpless in his experiment more easily than did internals (see also Dweck, 1975). So three independent factors, the laboratory procedure of uncontrollability, the cognitive set induced by chance instructions, and externality, each produced a learned helplessness effect. Given this convergence, Hiroto concluded that these three factors all erode motivation to escape by contributing to the expectation that responding and relief are independent.

This concludes the survey of motivational deficits produced by exposure to uncontrollable aversive events in different species. It seems to be generally true that uncontrollability produces deterioration in the readiness of dogs, cats, rats, fish, monkeys, and men to respond actively to trauma.

Generality. Does uncontrollability produce a habit limited to situations very like the ones in which uncontrollability is experienced, or does it produce a more general effect? The question we are really asking here is "Is helplessness just an isolated set of habits or does it involve a more basic change?" We believe that what is learned when the environment is uncontrollable can have consequences for a wide range of behavior.

At the lowest level of generality, helplessness transfers from some types of apparatus to others, as long as shock occurs in both situations; dogs given inescapable shock in a hammock fail to escape later in a shuttle box. But does what is learned transfer to aversive situations not involving shock? Braud et al. (1969) used a triadic design with mice. One group could escape shock by climbing up a pole, a second group was yoked, and a third group received no shock. All groups were then placed in an alley flooded with water and had to swim out in order to escape. The yoked group was poorest at escaping from water. (See McCulloch & Bruner, 1939, for similar findings in the rat, also for the earliest apparent learned helplessness study in the literature.) Rosellini and Seligman (1975) performed an experiment in which shock-induced learned helplessness may have transferred to frustration. Three groups of rats received escapable shock, inescapable shock, or no shock. After this pretreatment, they learned to run down an alleyway in order to get food in a goal box. Food was present on every trial in the goal box. Once they had learned, food was no longer placed in the goal box. So during this extinction procedure, the rats ran down the alleyway into the goal box where they expected food, but found none. This has been shown to be a "frustrating" and aversive experience for a rat (Amsel, Rashotte, & MacKinnon, 1966). The rats were then given an opportunity to jump out of the goal box and escape this frustration. Rats that had experienced escapable shock or no shock escaped the goal box readily. Rats that had experienced inescapable shock did not escape from the goal box. (See Figure 5.) The effect of ex-

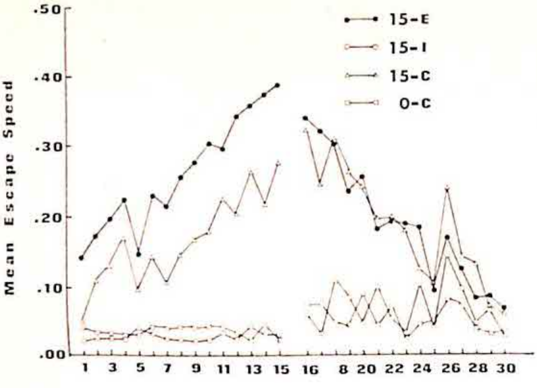

FIGURE 5. Mean speed to escape a nonrewarded goalbox. (The group designated by E previously received escapable shocks, the group designated by I received inescapable shocks, and the groups designated by C received no shocks. The groups designated by 15 received rewarded runway acquisition training, whereas the group designated by 0 did not. The rats given inescapable shocks [15-I] showed no tendency to escape the frustrating goalbox.) (From "Frustration and Learned Helplessness" by Robert A. Rosellini and Martin E. P. Seligman, *Journal of Experimental Psychology: Animal Behavior Processes,* 1975, *1,* 149–157. Copyright 1975 by the American Psychological Association. Reprinted by permission.)

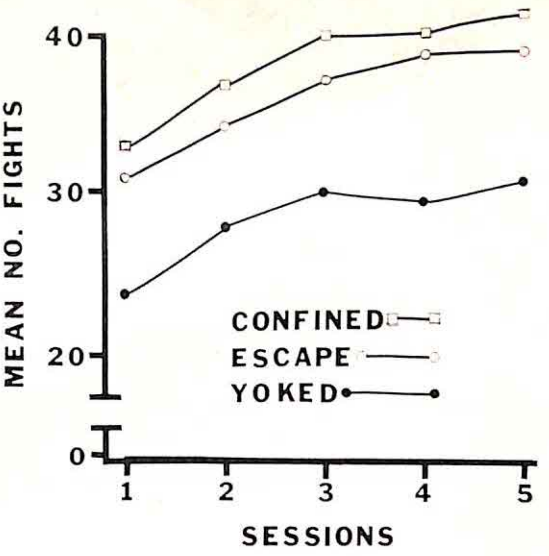

FIGURE 6. Shock-induced fighting frequency, for each of five test sessions, for rats that had received escapable, yoked inescapable, or no shocks. (Inescapable shocks depressed fighting.) (From "Influence of Control of Shock on Subsequent Shock-Elicited Aggression" by Steven F. Maier, Christine Anderson, and David A. Lieberman, *Journal of Comparative and Physiological Psychology,* 1972, *81,* 94–100. Copyright 1972 by the American Psychological Association. Reprinted by permission.)

posure to one kind of uncontrollable event generalized to another—frustration.

Another possible instance of the transfer of a learned helplessness effect is related to "shock-elicited aggression." If two rats are placed in a small enclosure and repeatedly shocked, the shocks frequently come to elicit attack and aggressive posturing. Maier et al., (1972) gave rats escapable shock, inescapable shock, or no shock, and then placed these rats in a shock-elicited aggression situation. As shown in Figure 6, the amount of shock-elicited aggression was lower in the rats previously exposed to inescapable shock than it was in the rats given escapable or no shock (also see Payne, Anderson, & Murcurio, 1970; Powell & Creer, 1969). In a related study (unpublished data) we found that dogs that had received inescapable shock as puppies lost out in competition for food (only one nose fits into a coffee cup full of Alpo) with dogs that received no shock or escapable shock. So it seems that exposure to uncontrollable aversive events retards the initiation of aggressive as well as defensive responses. (See Brookshire, Littman, & Stewart, 1961, Experiment 6; and Anderson & Paden, 1966, for other evidence on intra-aversive transfer.)

Does experiencing uncontrollable aversive events have effects on nonaversively motivated behavior? Recently, Hiroto and Seligman (1975) and Miller and Seligman (1975) systematically explored the transfer of helplessness between instrumental tasks and cognitive tasks. Three groups of college students received escapable, inescapable, or no loud noise. They were then switched to a nonaversive anagrams test, and time to solve a series of 20 anagrams like IATOP was recorded. Students who had received inescapable loud noise failed to find the solutions more often than the escape group and the no-noise group. So exposure to uncontrollable aversive events retards solution of cognitive problems in a nonaversive situation.

Are the debilitating effects of uncontrollability only produced by uncontrollable *trauma?* What happens to response initiation when preceded by a history of uncontrollable outcomes which are not traumatic? Hiroto and Seligman (1975) tried to produce a learned helplessness effect using unsolvable discrimination problems. Just as a solvable discrimination problem is controllable in the same sense that an escapable shock is controllable, an unsolvable discrimination problem is uncontrollable in the same sense that an inescapable shock is uncontrollable. The conditional probability of success given any response (e.g., to the left) is the same as the probability of success if the alternate response (e.g., to the right) is made.

With the formal similarity of unsolvability and inescapability in mind, Hiroto and Seligman (1975) and Klein et al. (in press) gave three groups of college students four sets of solvable, unsolvable, or no-discrimination problems. Then all groups were given the hand shuttle box with loud noise to be escaped. Individuals who had solvable discrimination problems or no prior problems escaped noise readily. The unsolvable group failed to escape the noise. So we believe that response initiation to control noxious events may be impaired by experience with uncontrollable reward.

Equally intriguing is the possibility that delivery of appetitive events independent of behavior may produce a failure to learn responses to procure such events, an effect analogous to learned helplessness. Engberg, Hansen, Welker, and Thomas (1973) trained one group of pigeons to press a treadle to obtain food. A second group of pigeons received food delivered independently of behavior, and a third group received only a small amount of magazine training. Following this pretreatment, all pigeons were tested with a key peck autoshaping procedure. In autoshaping (Brown & Jenkins, 1968) the pigeon key is periodically illuminated for a brief period of time, and food is presented at the end of each key illumination period whether or not a peck has occurred. Pigeons normally acquire key pecking under such an arrangement. The result was that the group given previous treadle press training was fastest to acquire key pecking and the group given noncontingent food was the slowest to acquire key pecking. Engberg et al. interpreted their results as constituting an appetitive analogue to learned helplessness; food was delivered independently of behavior, and this retarded the acquisition of a response to procure food.

However, the Engberg et al. experiment has been criticized on a number of grounds (Gamzu, Williams, & Schwartz, 1973). First, Gamzu et al. argued that autoshaping is not a reasonable task to use in attempting to establish an appetitive analogue to learned helplessness because autoshaped key pecks are largely controlled by Pavlovian contingencies (Moore, 1973) rather than instrumental contingencies; learned helplessness is a debilitation of instrumental responding. Second, Gamzu et al. noted that it is easy to account for the Engberg et al. experiment in terms of competing motor responses. Both the group given treadle training and the group given noncontingent food would have been expected to learn a motor response incompatible with key pecking (treadle responding and standing by the grain hopper, respectively). However, Engberg et al. removed the treadle during the autoshaping test, and so it could be argued that the stimulus controlling the competing behavior was removed for the treadle group. The stimulus controlling any competing behavior was not removed for the noncontingent food group, and this difference could account for the results. In response to these criticisms Welker (1974) conducted a further investigation of the phenomenon. One group of pigeons was trained to peck a key for food, a second was given noncontingent food, and a third was given only a small amount of magazine training. All pigeons were then given a test in which treadle pressing produced food on a continuous reinforcement schedule. The key was present during the treadle test, so it could not be argued that the stimulus controlling the competing behavior was removed for the contingent food group. The results were the same as in Engberg et al.; noncontingent food presentation retarded acquisition. Taking a hint from the helplessness experiments in rats (see pp. 8–9), Welker repeated his experiment with an FR-3 treadle response as the test task. That is, the treadle had to be pressed three times in order to produce food. This magnified the size of the observed effect.

A potentially related finding was recently reported by Bainbridge (1973). At 50 days of age, rats were given a solvable black–white discrimination problem, an unsolvable problem, or no problem. Twenty days later they were tested with the same tasks but different stimuli, or in a Hebb–Williams maze based on spatial cues. The rats given an initial unsolvable problem performed very poorly in both the same and the different apparatus. Thus exposure to appetitive events delivered independently of behavior may interfere with the acquisition of appetitively motivated responses, just as exposure to noncontingent shock interferes with the acquisition of shock-motivated responses.

So we believe that the psychological state produced by uncontrollability may undermine response initiation quite generally. Dogs, rats, cats, fish, and people make fewer responses to escape shock after receiving uncontrollable shock. Furthermore, these motivational deficits may not be limited to shock or even noxious events. Escape from frustration, aggressive behavior, and even the propensity to solve anagrams might be undermined by inescapable aversive events. Conversely, uncontrollable rewards disrupt escape from loud noise, learning to procure food, and competitiveness. However, it should be stressed that many of the experiments cited here are subject to alternative interpretation, and much more research will be required before the true extent of these effects will be known. Clearly, there must be a limit to the degree of generalization of learned helplessness effects, and these limits must be explored and delineated. We cannot yet say what factors will prove important. All that can be said now is that there appears to be some degree of transfer.

Cognitive Deficits

We have argued that a major consequence of experience with uncontrollable events is *motivational;* uncontrollable events undermine the motivation to initiate voluntary responses to control future events. Experience with uncontrollable events may also have a second consequence that is cognitive; experience with uncontrollability may produce a difficulty in learning that responses have succeeded, even when responding is actually successful. Uncontrollability may retard the perception of control.

This phenomenon seems to appear in helpless dogs, rats, and men. Occasionally, a naive dog sits and takes shock on the first three or four trials in the shuttle box, and then on the next trial jumps the barrier and escapes shock successfully for the first time. Once a *naive* dog makes one response that produces shock termination, it learns rapidly. On all further trials, the subject responds vigorously and goes on to learn to avoid shock altogether. But dogs that first received inescapable shock were different in this respect also. About one third of them go through a similar pattern—sitting through shock on the first three or four trials and then escaping successfully on the next. These dogs, however, then revert to taking the shock, and they fail to escape on future trials. It appears as if one success is just not enough to make a dog that has experienced inescapable shock learn that his responding now produces shock termination.

Miller and Seligman (1975) and Klein et al. (in press) found that such a negative cognitive set results from uncontrollability in man. Three groups of students received escapable, inescapable, or no loud noise. Then they confronted two new tasks, a task of skill and a task of chance. On each of 10 trials in the skill task, they sorted 15 cards into 10 categories of shape, attempting to complete it within 15 sec. Unknown to them, the experimenter arranged to have them succeed or fail on any given trial by saying that time was up before they had finished or after. So they went through a prearranged run of successes and failures. At the end of each trial, the subjects rated (on a 0–10 scale) what they thought their chances of succeeding on the next trial would be. Subjects who were previously exposed to inescapable noise showed very little change in their expectancy for successes after each new success and failure. They had difficulty perceiving that their response would affect succeeding or failing. Control subjects and subjects who had escaped noise showed large expectancy changes following each success and failure. This showed that they believed outcomes to be dependent on their actions. The three groups did not differ in expectancy changes following success and failure in a "chance" task that they perceived as a guessing game. So inescapable loud noise produced a cognitive set in which people believed that success and failure is independent of their own skilled actions within the experimental situation, and they therefore had difficulty perceiving that skilled responses were effective.

Hiroto and Seligman (1975), Miller and Seligman (Note 1) and Klein et al. (in press) also reported this negative cognitive set in another form. After escapable, inescapable, or no noise, as the reader will recall, students had to solve anagrams (see p. 11). Two kinds of deficits emerged. Inescapable noise interfered with their ability to solve any given anagram. In addition, there was a pattern to the 20 ana-

TABLE 1

Mean Number and Standard Deviation of Consecutive Successes Before Pattern Solution

Subject	Noise		
	Inescapable	Escapable	None
Depressed			
M	5.9	4.2	7.1
SD	2.53	1.36	1.22
Nondepressed			
M	6.9	4.5	3.7
SD	3.36	1.58	.86

Note. Adapted from Miller and Seligman (Note 1). Inescapable noise increased the number of successes required in nondepressed subjects.

grams to be solved; each was arranged with its letters in this order: 53124, for example, ISOEN, DERRO, OURPG, etc. As can be seen in Table 1, students who had received inescapable noise required about seven consecutive successes before catching on to the pattern; students who received escapable noise or no noise needed about three consecutive successes. Unsolvable discrimination problems, incidentally, produced the same disruption of anagram solution as did inescapable noise.

The existence of a negative cognitive set produced by independence between responding and outcomes bears on an important issue in learning theory. When two events are presented independently of each other, for example, a tone and a shock presented at random, does the subject learn anything at all about the tone or does he merely come to ignore it? A helplessness point of view holds that men and animals actively learn that responses and outcomes are independent of each other, and one way the learning is manifested is by the difficulty they later have learning that the response produces the outcome when it does. Merely substitute *shock* for *outcome* and *tone* for *response*. This suggests that organisms should also actively learn when a tone and shock are independent and that organisms show this by having trouble learning later that the tone is followed by the shock when it actually is. Rescorla (1967) held the contrary view. Independence between a tone and shock is a neutral condition in which nothing is learned. Recent investigation has shown that active learning does indeed occur when conditioned stimuli (CSs) and unconditioned stimuli (USs) are independently presented. Mellgren and Ost (1971) reported that a group for which CSs were presented independently of food took longer to learn later that the CSs were associated with the food than did naive rats (or even rats for whom the CSs predicted the *opposite* relationship with food). Kemler and Shepp (1971) showed that irrelevant stimuli were most slowly learned about when they became the relevant stimuli for the solution of a discrimination problem for children. Thomas, Freeman, Svinicki, Burr, and Lyons (1970) showed that pigeons that had two colors presented independently of food tended not to discriminate later between two line tilts, one of which predicted the presence of food and the other its absence. MacKintosh (1973) also reported retardation of conditioning by prior CS–US independence.

So independence between two stimuli may produce active learning, and this learning seems to retard the ability of rats, pigeons, and men to learn later that the stimuli depend on one another. This evidence coheres with the effects of response-outcome independence on cognition and bolsters the hypothesis that such independence retards the ability of the organism to perceive that his responding has contingent consequences (also see the discussion of experiments by Maier & Testa, 1975, pp. 24–25).

Emotional Deficits

Our first hint that uncontrollable aversive events had emotional as well as motivational and cognitive consequences came when we found that the motivational effects dissipated in time under some circumstances. Overmier and Seligman (1967) found that after one session of inescapable shock in the hammock, dogs were helpless in the shuttle box 24 hours later. If, however, the dogs were not tested in 24 hours, but at 48, 72, or 168 hours after inescapable shock, they were normal. This is the only circumstance we know under which helplessness dissipates in time. If multiple sessions of inescapable shock are given to the dog in the hammock, he will fail to escape a week later in the shuttle box (Seligman & Groves, 1970). Further, if the dog is reared from birth in laboratory cages, having minimal experience with control over events, small amounts of inescapable shock produce nontransient helplessness (Seligman & Groves, 1970). Finally, no time course has been observed in rats; following one session of inescapable shock, rats fail to escape 5 minutes, 1 hour, 24 hours, and 168 hours later. It must be borne in mind however, that under one circumstance—a single session of inescapable shock in dogs of unknown past history—helplessness dissipates in time, and such a time course hints of a transient emotional disturbance.

Other evidence indicates the emotional consequences of uncontrollability. One widely quoted study (Brady, Porter, Conrad, & Mason, 1958) is related to uncontrollability and helplessness, but appears to show *less* emotionality with uncontrollability. Two groups of four monkeys were given shocks; half of them—the "executives"—had control over the shock and could avoid them by bar pressing. The other four were yoked, or helpless, since they could not modify shock. The executives formed stomach ulcers and died, but the helpless monkeys did not. Unfortunately, this result may be an artifact of the way the monkeys were assigned to the two groups. All eight monkeys were placed on the executive schedule originally, and the first four to start the lever pressing became the executives. The last four became the yoked subjects. It is possible that the more emotional a monkey is, the more quickly it begins to bar press when it is shocked (Sines, Cleeland, & Adkins, 1963). So the four most emotional animals may have become the executives, and the four least emotional may have become the yoked subjects.

Jay Weiss (1968, 1971a, 1971b, 1971c) has recently repeated the executive monkey design with rats. (See also Moot, Ceballa, & Crabtree, 1970.) Three groups of rats were randomly assigned to the triadic design. The executive animals showed fewer and less severe ulcers than the yoked animals. Moreover, the yoked rats lost more weight, defecated more, and drank less than the executive. So rats given inescapable shock show more stress or emotional effects when measured by stomach ulcers than rats who can control shock.

There is further evidence in rats that uncontrollable shock produces more severe emotional reactions than does controllable shock. Mowrer and Viek (1948) shocked two groups of rats while the rats were eating. One group could control the shock by jumping in the air, and the other group received uncontrollable shock. The rats getting uncontrollable shock subsequently ate less than those controlling shock (see Brimer & Kamin, 1963; Desiderato & Newman, 1971; Lindner, 1968; and Payne, 1972, for an ongoing controversy surrounding these data). In an analogous human study by Hokanson, DeGood, Forrest, and Brittain (1971), subjects performed a symbol matching task while being shocked. The schedules were individually arranged so that each subject received an average of one shock every 45 sec. Subjects in the controllability group were allowed to take as many time-outs as they wished whenever they wanted. A yoked control group received the same number of time-outs at the same times. Measures of blood pressure taken at 30-sec intervals indicated that yoked subjects showed consistently higher blood pressures. (See Averill & Rosenn, 1972; Bandler, Madaras, & Bem, 1968; Corah & Boffa, 1970; Elliot, 1969; and Stotland & Blumenthal, 1964, for related studies in humans using a variety of other measures of emotional arousal. This is a complex and inconsistent literature and is reviewed by Averill, 1973.)

Hearst (1965) found that the presentation of uncontrollable shocks resulted in a breakdown of a well-trained appetitive discrimination in rats. During uncontrollable shock his rats no longer discriminated between two stimuli, one of which signaled the presence and the other the absence of food. During controllable shock the appetitive discrimination was maintained.

Such breakdown of appetitive discriminations is reminiscent of the classic work on "experimental neurosis." The concept of experimental neurosis is not a homogeneous one; neither is it well defined. Unlike the other studies reviewed, controllability has not been manipulated explicitly to produce the "neurosis." Yet from an operational point of view, we can speculate that the lack or loss of control may be important in their etiology. Typically, an animal is restrained in some type of harness seriously limiting what it can do. Often the procedure is Pavlovian, and by definition the organism has no control over the onset or the offset of the stimuli presented. In Shenger-Krestnikova's (in Pavlov, 1927) classic experiment, an appetitive discrimination deteriorated and signs of distress were noted when the dog could no longer tell the difference between the rewarded and nonrewarded stimulus. In the work of Liddell, James, and Anderson (1934), sheep developed a range of maladaptive behaviors following uncontrollable electric shock. Masserman (1943) trained monkeys to feed in response to a signal and made them "neurotic" by presenting a fear-arousing stimulus during feeding. Without "therapy" these monkeys remained disturbed almost indefinitely. According to Masserman (1943):

> Markedly different, however, was the case of animals that had been taught to manipulate various devices that actuated the signals and feeder because in this way they could exert at least partial control over their environment. This stood them in good stead even after they were made neurotic in as much as when their hunger increased they gradually made hesitant, but spontaneous, attempts to reexplore the operation of the switches, signals and food boxes, and were bolder and more successful as food began to reappear. (p. 82)

In a dramatic primate study, Stroebel (1969) trained a group of rhesus monkeys to air-condition their overheated chamber by pressing a lever, and also to control loud noise, annoying light, and mild shocks by pressing the same lever. He then retracted the lever so that it could still be seen, but could no longer be pressed. *No further physical stressors were presented.* Initial responding was frantic. Brain temperature became irregular and the circadian rhythm was disrupted.

> As rhythm disturbance developed, members of this . . . group of subjects began to show lassitude and weakness; their fur became knotted, mottled and poorly groomed; behaviorally they performed unpredictably if at all on the right hand lever problems, pausing often for naps and rest. The behaviors exhibited by these animals were clearly nonadaptive in nature; for example, two subjects spent hours in catching "imaginary" flying insects, one subject masturbated almost continuously, three subjects became almost compulsive hair pullers, and all tended to show movement stereotypy alternating with an almost total lack of interest in their external environment. (Stroebel, 1969, p. 97)

It is not clear whether there can be any one theory which can account for "experimental neuroses," nor is it clear whether all these phenomena are closely related. But uncontrollability is prominently present, and emotional disruption is the frequent result.

In summary, three types of disruption seem to be caused by uncontrollability of aversive events in the laboratory: The motivation to respond is reduced, the propensity to perceive success is undermined, and emotionality is modified. These effects hold across a variety of situations and species. We need a theory to put all this together and we will now propose one.

LEARNED HELPLESSNESS THEORY

The basic facts about learned helplessness effects in the laboratory have now been arrayed before the reader. What must an adequate theory of helplessness accomplish? It must account for the three facets of the effects of uncontrollable aversive events: motivational, cognitive, and emotional. It must be testable; there should be experiments that can be performed which would confirm it if it is true, or disconfirm it if it is false. Finally, it must be applicable outside the laboratory—it must be useful in explaining any helplessness effects found in the real world. This final requirement is beyond the scope of this article, but the interested reader should see Seligman (1975).

The theory to be presented is straightforward, and the groundwork for it has been prepared by the way we laid out the data above. It accounts directly for the motivational and cognitive deficits and, with an additional premise, can account for the emotional disturbance. The theory has been tested in several ways, and as a bonus it predicts ways to eliminate helplessness effects and ways to prevent them.

When an organism is faced with an outcome that is independent of his responses, he sometimes learns that the outcome is independent of his responses.

This is the cornerstone of our view and probably seems obvious to all but the most sophisticated learning theorist. You will recall our discussion of the response contingency space (Figure 1). Learning theorists would much prefer that the kinds of contingencies that can be learned about be as simple as possible. First they believed that the most that could be learned about was a simple pairing of a response and an outcome, and the pairing of the response with the absence of the outcome. But this had to be broadened to include partial reinforcement, with the subject integrating both kinds of pairings. So what could be learned about was broadened to the probability of an outcome given a response. Then, it was shown that organisms could also learn about the probability of an outcome given that it did not make the indicated response. The added step which our view makes is that the organism can learn about both these probabilities conjointly, that variation of experience corresponding to the points in the response contingency space will produce systematic particular, we have argued that exposing changes in behavior and cognition. In organisms to the 45° line, in which the probability of the outcome is the same whether or not the response of interest occurs, produces learning. Behaviorally, this learning should tend to produce lack of response initiation to control the outcome; cognitively it should produce a belief in the inefficacy of responding and difficulty in learning that responding succeeds: and emotionally when the outcome is traumatic it might produce emotional changes.

The basic triadic design employed in all the helplessness studies reviewed above is, of course, directly relevant to the premise that men and animals learn about, and form expectations concerning, independence between outcome and responses. So, for example, in the Seligman and Maier (1967) study only the yoked dogs later failed to escape shock, while the dogs who could escape by panel pressing and the dogs who were not shocked did not fail to respond later. Clearly something different happened to the dogs that received shock independently of their responses. We believe they learned that responding was futile and therefore expected future responding to shock to be futile. We have gone to pains to give an *objective* definition of *uncontrollability* and *response–outcome independence*. This is because the theory is basically a three-step affair.

Information about contingency →
Cognitive representation of the contingency
(learning, expectation, perception, belief) →
Behavior

An organism must begin with the information about the contingency between response and outcome. This information is a property of the world out there, a set of stimuli, and not a property of the receiver. We have carefully defined the kind of information that can be called information that a response and an outcome are independent.

But the middle step in the chain is crucial and is easily overlooked. The information about the contingency must be processed and transformed into a cognitive representation of the contingency.[3] Such a representation has been variously called "learning that outcome and response are independent," "perceiving that response and outcome are independent," or "believing that response and outcome are independent." We prefer to call the representation "the expectation that responding and an outcome are independent."

A person or animal can be exposed to the environmental contingency in which an outcome response are independent, yet not form such an expectation. Immunization, as we shall see later in this article (refer to p. 25), is an example. Conversely, a person can show a helplessness effect without being exposed to the contingency as such; he can merely be told that events are uncontrollable.

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[3] For attempts to spell out in detail the relationship between the contingency information and its cognitive representation, the interested reader should consult, for example, Kelly (1967, 1972), Weiner, Frieze, Kukla, Reed, Rest, and Rosenbaum (1971) from an attribution theory point of view; Irwin (1971) and Seligman and Johnston (1973) from a cognitive-learning theory point of view; as well as Lazarus (1966) and Stotland (1969).

Glass and Singer (1972) have done an extensive series of studies on the role of controllability in reducing stress and have found that merely telling a human subject about controllability duplicates the effects of actual controllability. They attempted to mimic the stress of the urban environment by having their subjects (college students) listen to a very loud melange of sound: two people speaking Spanish, one person speaking Armenian, a mimeograph machine, a calculator, and a typewriter. When subjects could *actually* turn off the noise by button pushing, they were more presistent in problem solving, they found the noise less irritating, and they did better at proofreading than yoked subjects.

Glass and Singer also presented another group of subjects with the same noise, but this time it was uncontrollable. However, this group had a panic button and was told: "You can terminate the noise by pressing the button. But we'd prefer you not do it." None of the subjects in fact tried to turn off the noise. All they had was the false information that they could control the noise if they had to. These subjects did just as well as the subjects who actually controlled the noise. Actual controllability and actual uncontrollability may often produce corresponding expectations. But this type of experiment, in which the expectation is invalid, highlights the fact that it is the expectation and not the objective conditions of controllability that is the cornerstone of our theory. (See also Geer, Davison, & Gatchel, 1970; Geer & Maisel, 1972; Klein et al., in press; Langer, 1974; Pervin, 1963; and Stotland & Blumenthal, 1964, for related effects of perceived control.)

So the first step of the theory is that the organism acquires an expectation of response-outcome independence, when outcomes are uncontrollable. The second step in the theory is the *means* by which the expectation of response–outcome independence produces the effects associated with helplessness.

We assume that the incentive to initiate voluntary responses in a traumatic situation is partly produced *by the expectation that responding produces relief. In the absence of this incentive, voluntary responding will decrease in likelihood. When a person or animal has learned that relief is independent of responding, the expectation that responding produces relief is negated, and therefore response initiation is reduced.* Most generally put, the incentive to initiate voluntary responses to control any outcome (e.g., food, sex, shock termination) comes, in part, from the expectation that responding produces that outcome. (See Bolles, 1972, for a discussion of expectational mechanisms and incentive motivation.)[4] In the absence of this incentive, voluntary responding will decrease in likelihood. When a person or animal has learned the outcome is independent of responding, the expectation that responding will produce the outcome, and therefore response initiation, decreases.

The way in which this undermining of motivation works has been seen with crystal clarity in a human helplessness experiment (Thornton & Jacobs, 1971). Following inescapable shock, college students sat and took the shock. When asked why they did not respond appropriately, *60% of the subjects reported they had no control over shocks, so why try.* These subjective reports suggest that a belief in uncontrollability undermines the incentive to initiate responses.

We also assume that *learning that an outcome is independent of a response makes it more difficult later to learn that responses produce that outcome.*

Response–outcome independence is an active form of learning, and like any other active form of learning, it can proactively interfere with contravening forms of learning through an associative interference. At first the dog is not a passive recipient of shock but emits many responses, but each of the responses is unrelated to shock termination. The dog might, for example, turn his head and shock might happen to go off on the trial, but on other trials he might turn his head and shock would not go off. Shock would also terminate when he had not turned his head. Now when he is tested in the shuttle box and jumps the barrier, which in reality causes shock termination, the dog has trouble learning this. This is because he has had many experiences in which a pairing between a response and shock termination proved spurious and, as for head turning, he still expects that shock will just as likely go off if he fails to jump the barrier. Such a dog may return to taking shock even after he makes one or two "successful" jumps. In contrast, a naive dog has no interfering expectation that shock termination is independent of responding, so one experience with barrier jumping leading to shock termination is sufficient for him to learn. For some evidence regarding such associative interference see the discussion of Maier and Testa (1975), pp. 24–25.

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[4] It should also be mentioned that innately elicited struggling is another source of responding in a traumatic situation, but it is the waxing and waning of *voluntary* (outcome-sensitive) responses which is our concern here. This does not deny that innate responses can be transformed into voluntary responses (Schwartz & Williams, 1972).

This, then, is the theory of helplessness. The expectation that an outcome is independent of responding (a) reduces the motivation to control that outcome and (b) interferes with learning that responding controls the outcome.

Seligman (1975) has recently extended this theory to changes in emotionality. He argued that when a traumatic event first occurs it causes a heightened state of emotionality, which has been called "fear." The fear continues until the subject learns that he can or cannot control the trauma. If the subject learns he can control the trauma, fear is reduced and may disappear altogether. If the subject learns that he cannot control the traumatic event, fear decreases and is replaced with depression. For a more detailed discussion see Seligman (1975).

ALTERNATIVE EXPLANATIONS

Now that we have described the learned helplessness hypothesis and the manner in which it is able to account for the phenomena under consideration, we will describe some theoretical positions which have been advanced as alternatives to the helplessness hypothesis. It should be noted at the outset that these hypotheses have been offered as explanations of only the fact that organisms exposed to inescapable shock later fail to learn to escape and avoid shock in a shuttle box, rather than as explanations of the broad range of phenomena considered above. Thus we will describe these theoretical positions in the context of the basic learned helplessness effect. These alternative theories can be divided into two groups, motivational accounts and motor accounts.

Motivational Alternatives

Adaptation. The adaptation hypothesis maintains that a subject adapts to shock during pretreatment with inescapable shocks and is therefore not sufficiently motivated to escape from shock in the shuttle box. The hypothesis is inadequate:

1. Adaptation to repeated, intense shock has never been demonstrated (Church LoLordo, Overmier, Solomon, & Turner, 1966).

2. It is unlikely that very much adaptation could persist for as long as 24 hours and beyond.

3. In experiments with dogs, the dogs do not look as though they are adapted; they howl, defecate, and urinate to the first shock presentation in the shuttle box. On later trials, the dogs are passive; but they whimper and jerk with the shock.

4. We have disconfirmed the adaptation hypothesis experimentally. Raising the shock level in the shuttle box should increase motivation to escape. However, Overmier and Seligman (1967) found that increasing the shock level from 4.5 mA to 6.5 mA does not eliminate the interference effect.

5. A series of escapable shocks in the hammock does not produce failure to escape in the shuttle box (Seligman & Maier, 1967), while the same shocks, if *inescapable,* do produce failure to escape. By this hypothesis, both conditions should lead to equal adaptation to shock and to similar behavior in the test situation, but they do not.

6. Seligman and Maier (1967) gave one group of dogs 10 trials of escape/avoidance training in a shuttle box *before* treatment with inescapable shock in the harness. Following exposure to inescapable shocks in the harness, the dogs escaped and avoided shock normally when returned to the shuttle box. Thus prior exposure to controllable shock "immunized" the dogs against the interfering effects of exposure to inescapable shocks. This result follows from the helplessness hypothesis because prior experience with controllable shock should proactively interfere with the subject's learning that shock is uncontrollable and should also allow the subject to discriminate between the places where shocks are controllable and uncontrollable. However, this result is not consistent with the adaptation hypothesis. Prior exposure to escapable shock in the shuttle box should not eliminate any adaptation produced by inescapable shocks in the harness.

7. If the subject is tested and fails to escape 24 hours after inescapable shocks, the learned helplessness effect will persist in chronic form; the subject will fail to escape on later opportunities. The helplessness hypothesis suggests a way to eliminate chronic failure to escape. By this hypothesis, the dog does not try to escape because he does not expect that any instrumental response will produce shock termination. By forcibly exposing the dog to the escape and avoidance contingencies, this expectation might be altered. This type of training by "putting through" has been used by others with mixed success (Loucks, 1935; Miller & Konorski, 1928; Woodbury, 1942). Seligman et al. (1968) reasoned that forcibly dragging the dog from side to side in the shuttle box, in such a way that the dog's changing compartments terminated shock, might effectively expose the dog to the response-reinforcement contingency. This was the case. The experimenter pulled three chronically helpless dogs back and forth across the shuttle box with long leashes. This was done during CS and shock, while the barrier was absent. After being pulled across the center of the shuttle box (and thus terminating shock and CS) 20, 35, and 50 times, respectively, each dog began to respond on his own. Then the barrier was replaced, and the subject continued to escape and avoid. The recovery from helplessness was complete and lasting. This type of exposure to the escape and avoidance contingencies should not affect any adaptation to shock that might have been present. Seligman et al. (1975) replicated these findings in rats.

Sensitization. Perhaps the inescapable shocks received in the harness sensitize the subject to shock so that it is too motivated to enable it to make organized responses in the shuttle box. This hypothesis is inadequate. (a) Sensitization explains the inefficiency of responding but not the absence of responding. (b) Lowering the shock level in the shuttle box should permit the subject to make organized responses. However, Overmier and Seligman (unpublished data) found that the interference effect is not attenuated when shock in the shuttle box is reduced to 3.0 mA. (3) Arguments 5, 6, and 7 in the previous paragraph, which invalidate the adaptation hypothesis, also invalidate the sensitization argument.

Motor Activity Alternatives

Within the last 2 years three hypotheses have been offered as explanations of the learned helplessness effect which are more difficult to dismiss than are sensitization and adaptation. These explanations have the advantage that they are able to account for the fact that the learned helplessness effect is determined by the controllability of the shock during the initial treatment. We have classified them as motor activity hypotheses because they all maintain that exposure to inescapable shock interferes with subsequent shuttle box acquisition because it changes motor activity. Two of these hypotheses argue that exposure to inescapable shock establishes a motor response that is incompatible with shuttling and therefore competes with shuttling. Thus these two hypotheses argue that the organism does not learn shuttling because it is performing a motor response that prevents the occurrence of shuttling. The remaining hypothesis maintains that inescapable shock is a powerful stressor and depletes a neurochemical necessary for the occurrence of movement. Thus this view holds that inescapably shocked animals fail to acquire shuttling because they cannot move sufficiently.

Incompatible motor response theories. As already noted, two different incompatible motor response explanations of the learned helplessness effect have been proposed. The first to be described was offered by Bracewell and Black (1974) and was suggested by three experiments. In their first experiment they examined the effects of restraint and inescapable shock upon simple FR-1 shuttlebox escape/avoidance acquisition. They found that restraint in the absence of any exposure to inescapable shock produced a decrement in FR-1 shuttlebox acquisition, but that exposure to inescapable shock in the absence of restraint did not produce a reliable decrement. In their second experiment Bracewell and Black found that exposure to "high" intensity (1.0 mA) inescapable shock produces a small decrement in FR-1 shuttle box acquisition even when delivered in the unrestrained condition. In their final experiment Bracewell and Black explicitly punished movement by arranging a positive correlation between movement and shock intensity. This procedure led to a decrement in subsequent FR-1 shuttle box acquisition.

Bracewell and Black (1974) proposed the following interpretation of their data:

> One explanation that accounts for these results is the operant conditioning of responses incompatible with the shuttle box escape response that occurred in the third case [where movement was explicitly punished], also occurred in the first two. That is, restrained rats may have been punished for struggling and consequently, learned to hold still, and unrestrained rats may have learned to hold still or to perform some other incompatible response to reduce the high intensity shock. (p. 67)

Thus Bracewell and Black (1974) argued that because explicit punishment of movement produced a retardation in shuttle box acquisition, the other instances of retarded acquisition might also be due to punishment of movement during the pretreatment phase. They went on to argue that the learned helplessness effect might be explainable in the same terms. It should be noted that this hypothesis can explain why escapable shock does not produce a subsequent decrement. If shock is escapable, the organism learns a response that completely eliminates shock (the escape response) and so does not have to learn a response that only mitigates shock (the putative incompatible response). It need only be argued that the former is not incompatible with shuttling but the latter is.

We will present a large amount of data which we feel to be inconsistent with the Bracewell and Black (1974) hypothesis. However, these experiments also bear on the second competing motor response theory and so they will not be presented until that theory has been described. Nevertheless, a few comments are appropriate here. First, the fact that explicit punishment of movement produces a decrement in subsequent escape/avodiance acquisition does *not* imply that other procedures which produced escape/avoidance decrements do so because they punish movement. There are undoubtedly many ways to produce poor escape acquisition, and it is unlikely that all operate through a similar mechanism. Paralyzing the animal with a drug would retard escape acquisition, but this does not mean that the debilitating effect of inescapable shocks occurs because inescapable shock induces paralysis. Thus the retarding effect of movement punishment does not imply that inescapable shock retards acquisition because it punishes movement. Moreover, the explicit punishment of movement in the Bracewell and Black experiment led to a *much smaller* escape acquisition decrement than did inescapable shock delivered to restrained subjects. The punishment procedure increased the subsequent mean escape latency across the 10 test trials from the control level of 2.4 sec to 9.1 sec. The inescapable shock procedure resulted in a 21.5-sec mean escape latency. So it can be argued that the explicit training of an incompatible response did not duplicate the effects of exposure to inescapable shock in restrained subjects, and so the argument that the movement punishment effect accounts for the inescapable shock effect seems questionable. It can, of course, be argued that a more effective or different punishment procedure might yield a larger escape decrement, but that remains to be demonstrated.

Further, it is not clear that Bracewell and Black (1974) employed a test procedure that is generally sensitive to learned helplessness effects. In order for an effect to be called a learned helplessness effect, it must be demonstrated that the effect is caused by the *controllability* of the events delivered during pretreatment. There is no evidence that decrements in *FR-1* shuttle box acquisition produced by prior exposure to shock are caused by the inescapability of the shocks in rats. There is not a single study employing a triadic design in rats (see p. 6) that has found appropriate effects with FR-1 shuttling as the dependent variable. In fact, there have been both reported (Anderson, Schwendiman & Payne, Note 3; Maier et al., 1973; Seligman & Beagley, 1975) and unreported (Carder, Leaf, Note 4) failures to find *any* effect of prior exposure to shock on FR-1 shuttle box escape acquisition in the rat. It is to be noted that both Anderson et al. and Maier et al. (1973) delievered inescapable shock to restrained rats and still failed to observe any effects. There are a number of differences between the various FR-1 shuttle box experiments, and it is not known which are responsible for the difference in results. Nevertheless, the dependent variable used by Bracewell and Black may not be one that is sensitive to helplessness effects in rats and so the implications of their results for learned helplessness is difficult to assess.

A further problem concerns the generality of the effects found by Bracewell and Black (1974). It will be recalled that they found restraint in the absence of shock to produce a subsequent escape deficit and inescapable shock in the absence of restraint to produce only a small escape decrement. However, Maier et al. (1973) used an escape/avoidance task known to be sensitive to helplessness effects in rats (see p. 8) and found no effect of restraint alone. Cohen (1970) found no effect of restraint on shuttle box escape/avoidance acquisition in dogs. Further, Looney and Cohen (1972), Seligman and Beagley (1975), and Seligman et al. (1975) reported large escape deficits produced by exposure to inescapable shock in unrestrained rats. Again there are many differences between studies, but it is clear that restraint does not always produce a subsequent escape decrement nor does inescapable shock have to be delivered to restrained subjects for a large effect to occur.

A second incompatible motor response theory has been proposed by Anisman and Waller (1973). They argued that exposure to shock induces response repertoire changes in the organism, and if no coping response is available, "freezing" becomes the organism's dominant reaction to shock. Freezing would, of course, compete with the occurrence of active responding in a subsequent escape/avoidance task. As evidence for their hypothesis, Anisman and Waller cite a variety of studies which show that manipulations designed to increase freezing have a detrimental effect on avoidance behavior, and manipulations designed to reduce freezing facilitate avoidance performance. So, for example, strong shock, which causes more freezing in rats, produced more interference with shuttle avoidance than weak shock (Anisman & Waller, 1972) and scopalamine, a drug that reduces freezing, makes rats better avoiders (Anisman, 1973).

A number of comments are appropriate. First, the experiments discussed by Anisman and Waller are not directly relevant to the learned helplessness effect. They are all studies of effects on *avoidance,* not escape. There is no evidence that the manipulations described by Anisman and Waller affect *escape* acquisition. In fact, escape latencies are not even reported in many of the experiments (e.g., Anisman, 1973). The learned helplessness effect is a failure to escape—to flee from shock itself—a more profound debilitation than failure to prevent shock. Second, even if it could be shown that inducing freezing debilitates escape, this does not mean that inescapable shock interferes with escape by inducing freezing. In addition, dogs do not seem to freeze, people who receive unsolvable discrimination problems or inescapable noise do not freeze, and organisms that receive noncontingent food do not freeze, yet these conditions may all produce helplessness. Finally, why does inescapable shock, but not escapable shock, produce freezing (if it does) in rats? We have not been able to think of an answer that does not imply that the rat has learned that shock is inescapable, and this is the heart of the helplessness view.

Experimental evidence. We feel that there are six lines of evidence that bear on the incompatible motor response theories, and each will be described.

1. It has already been noted that rats exposed to inescapable shock often do not fail to learn an FR-1 shuttle box response. This has been investigated by Maier et al. (1973), and in agreement with a number of previous investigators they found that rats exposed to inescapable shock under a wide range of parameters subsequently responded as rapidly as did controls in a two-way shuttle box escape/avoidance acquisition test. However, they noted that the rats in their experiments seemed to acquire this shuttling escape response in a manner different from that exhibited by dogs. The rats responded very rapidly, but the response was as rapid on the very first acquisition trial as it was after 30 trials—the acquisition curves were flat. The mean response latency from shock onset on the very first shuttle box trial was 2.67 sec. The escape responding of dogs in a shuttle box is quite different. Averaged across experiments, the naive control dogs in the Maier, Overmier, and Seligman experiments took slightly over 30.0 sec of shock before escaping on the first trial and did not emit short latency escape responses until the fourth trial. This suggested to Maier et al. (1973) that an interference effect in rats might be found if a more slowly acquired escape response were used as the test task. The escape response they used was one in which the rats had to cross the shuttle box *twice* in order to terminate shock. However, these FR-2 trials were preceded by five ordinary shuttle box trials (FR-1), since pilot investigation revealed that naive rats sometimes fail to acquire the FR-2 shuttling response if such prior training was not provided. The result was that a large interference effect appeared—half of the rats exposed to inescapable shocks did not learn the FR-2 escape response even though they responded rapidly on the five FR-1 trials, while all control rats learned the FR-2 escape response. It should be noted that we have replicated this effect several times, as have other laboratories (Seligman & Beagley, 1975; Weisman, Note 5).

The fact that rats exposed to inescapable shock will learn FR-1 shuttling but will not learn FR-2 shuttling might be explained in a variety of ways. The helplessness hypothesis asserts that prior exposure to inescapable shock interferes with the subsequent formation of associations between responding and shock termination and, in addition, reduces the organism's incentive to attempt to escape. From the foregoing discussion it is clear that crossing the shuttle box *once* is a high probability initial response to shock in the rat, perhaps a species-specific defense response (Bolles, 1970) and, at least on early trials, may be elicited. Consistent with this view of shuttle box crossing, Maier et al. (1973) found that if conditions were arranged such that there was no contingency between crossing the shuttle box and shock termination, naive rats would still cross the shuttle box in response to shock with short latencies for about 10 trials.

It should be noted that this strong tendency of the rat to cross the shuttle box rapidly in response to shock guarantees that the preshocked rats will be exposed to the FR-1 contingency a relatively large number of times even if such a rat is slow to learn. In addition, the contingency between crossing the shuttle box and shock termination is relatively simple under the FR-1 procedure, and so the preshocked rats might learn FR-1 escape even though associative interference is actually present. If associative interference were present, some difference in FR-1 acquisition between inescapably shocked and control rats, at least on early trials, might be expected. However, such a difference could be masked by the elicited nature of shuttling on early trials. Consistent with this argument, Testa, Juraska, and Maier (1974) found that FR-1 shuttling *extinguished* more rapidly in groups given previous exposure to inescapable shock than in controls, even though these groups did not differ in the acquisition of FR-1 shuttling.

The FR-2 shuttling response, on the other hand, is not as probable an initial unconditioned response to shock as is FR-1 shuttling, and presents a more complicated contingency between crossing the shuttle box and shock termination than does FR-1 shuttling. Thus the associative interference produced by learning that shock is inescapable might be able to prevent the acquisition of can also explain why exposure to inescap- the FR-2 shuttle box response.

Both competing motor response hypotheses able shock has little or no effect on FR-1 shuttling. The competing motor response theories could argue that the competing response is not strong enough to interfere with the highly elicited FR-1 but can compete with the initially weaker FR-2. In addition the FR-2 procedure exposes the rat to more shock than does the FR-1 procedure, and it could be argued that this intensifies the competing response.

Although these hypotheses are not mutually exclusive, they point to different aspects of the FR-2 procedure as the crucial feature in producing an interference effect. The helplessness hypothesis emphasizes the difficulty of learning the contingency between crossing the shuttle box and shock termination inherent in the FR-2 procedure, while the competing response explanations emphasize the physical difficulty of the FR-2 response and/or the large amount of shock to which the FR-2 procedure exposes an organism. There are two obvious ways to determine which of these aspects of the FR-2 procedure is crucial. The first is to simplify the contingency between crossing the shuttle box and shock termination while at the same time maintaining the physical difficulty and shock exposure characteristics of the FR-2. The second is to arrange a complex contingency between shuttling and shock termination while at the same time reducing the physical difficulty of the response and the extent of shock exposure. Maier and Testa (1975) have conducted experiments using each of these strategies.

The contingency between crossing the shuttle box and shock termination should be relatively difficult to learn with the FR-2 procedure, for the obvious reason that shock does not terminate after each crossing. In the Maier et al. (1973) FR-2 experiment, shock was simultaneously presented to the grids of *both* chambers of the shuttle box, so that there was no consequence for the first response of the FR-2, not even a momentary interruption of shock presentation. The contingency between the shuttle response and shock termination can be improved by arranging a brief interruption of shock following the first response in the FR-2. It should be noted that such a procedure will not change the effortfulness of the FR-2 nor will it have an appreciable effect on the duration of shock exposure produced by the FR-2 procedure, if the interruption in shock is very brief.

Maier and Testa (1975) conducted precisely the experiment suggested above. It was found that although inescapably shocked rats fail to learn ordinary FR-2 shuttling, they learn as well as nonshocked controls when there is a 1-sec interruption of shock after the first crossing of the FR-2. Since the only obvious difference between the procedures which did and did not yield a learned helplessness effect was the nature of the contingency between responding and shock termination, it seems likely that associative factors are strongly involved.

The previous experiment suggests that the inescapably shocked rats learn the FR-1 task but do not learn the FR-2 task because the FR-1 task contains an obvious contingency between shuttling and shock termination while the FR-2 task contains a degraded contingency. If this is the case, then it ought to be possible to produce a learned helplessness effect with a single crossing of the shuttle box if the contingency between the single crossing and shock termination were degraded.

In a second experiment, Maier and Testa (1975) degraded the contingency between a single crossing of the shuttle box and shock termination by interposing a time delay between crossing of the shuttle box and shock termination. Thus the required escape response was no more effortful than the ordinary FR-1 task which does not

TABLE 2

DESIGN OF THE MAIER AND RHOADES EXPERIMENT

Group	Phase 1	Phase 2	Phase 3
ES-IS	Escapable wheel turn	Inescapable tube	Escapable shuttle box
IS-IS	Inescapable wheel turn	Inescapable tube	Escapable shuttle box
NS-IS	No-shock wheel turn	Inescapable tube	Escapable shuttle box
NS-NS	No-shock wheel turn	No-shock tube	Escapable shuttle box

yield an interference effect, but contained an obscured contingency between shuttling and shock termination, as does the FR-2 task which does yield an interference effect. We found that a delay in shock termination as short as 1 sec severely retarded the acquisition of FR-1 shuttle box escape in rats previously exposed to inescapable shocks, but had no effect on subjects not previously exposed to inescapable shocks. It should be noted that a procedure designed to control for the extra amount of shock produced by the delay revealed that this factor did not account for the results. Thus prior exposure to inescapable shock will retard the acquisition of even low-effort escape responses if the contingency between the escape responses and shock termination is made less obvious.

Taken together, the results of these two experiments indicate that the nature of the contingency between the escape response and shock termination is more important than are the effort and shock exposure characteristics of the escape response in determining whether an interference effect will occur. Inescapably shocked rats fail to learn FR-2 shuttle box escape because the contingency between crossing the shuttle box and shock termination is here more complex, not because the FR-2 response is effortful or exposes the subject to long shock durations. It is therefore difficult to account for the rat interference effect with an explanation that does not posit an associative deficit following inescapable shock.

The incompatible motor response hypotheses cannot handle these data. It is difficult to conceive of a motor response that would interfere with FR-2 shuttling when there is no shock interruption, but not when there is. Similarly, what motor response could be incompatible with FR-1 shuttling if there is a short delay in shock termination but not incompatible when there is no delay?

2. It will be recalled (p. 20) that Seligman and Maier (1967) found that prior exposure to controllable shock immunizes the organism against the deleterious effects of exposure to uncontrollable aversive events. This finding might seem inconsistent with the incompatible motor response hypotheses since prior exposure to escapable shock should not prevent the establishment of the incompatible shock-mitigating response or freezing during exposure to uncontrollable shock. However, in the Seligman and Maier experiment the subjects' initial experience with controllable shock was given with the same task that was used as the test task following exposure to inescapable shock. That is, the subjects were first given escape training in a shuttle box, then given inescapable shock in a harness, and then tested in a shuttle box. Thus the immunization effect may not have been caused by the organisms' initial control over shock but rather by the acquisition of the specific response later used as a test.

The confounding of initial control over shock with the acquisition of the test task response allows the incompatible motor response theories to account for the Seligman and Maier immunization experiment. They could argue that the Seligman and Maier procedure strengthened shuttling sufficiently to overcome the putative competing response.

Clearly, what is needed is an experiment which separates the effects of initial control over shock and the prior strengthening of the specific response later to be tested. An unpublished experiment by Maier and Rhoades meets this requirement. (See also Seligman et al., 1975). The design of the

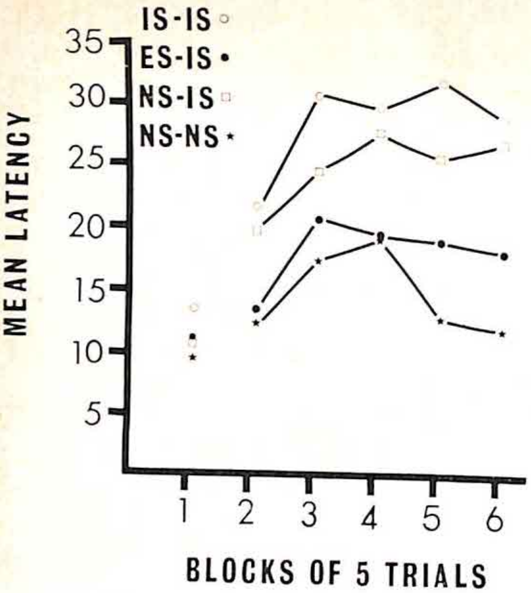

FIG. 7. Mean shuttle box FR-2 escape response latency for rats previously given either escapable shock (ES), yoked inescapable shock (IS), or no shock (NS) in a wheel-turn apparatus, and then given either inescapable shock (IS) or no shock (NS) in a restraining tube (Maier & Rhoades, unpublished data, 1975). (The rats given escape training in the wheel-turn box and then given inescapable shocks in the restraining tube [ES-IS] were not helpless in the shuttle box.)

experiment can be seen in Table 2. The experiment involved three phases and four groups of rats. In the first phase one group of rats was trained to escape shock by turning a small wheel with their paws, a second group received yoked inescapable shock in a wheel-turn box, and two groups were placed in the wheel-turn box but not shocked. In the second phase both of the shocked groups and one of the control groups received inescapable shock in a restraining tube, and the remaining group was only restrained. In the third phase all groups were given five trials of FR-1 followed by 25 trials of FR-2 escape/avoidance training in a shuttle box, our standard helplessness test task.

The results found by Maier and Rhoades are seen in Figure 7. Prior experience with escapable shock in the wheel-turn situation mitigated the interfering effects of inescapable shock on shuttle box escape acquisition. Although prior experience with escapable shock in the wheel-turn situation did not completely eliminate the effects of exposure to inescapable shock, the immunizing effect was reliable. It is difficult for competing response theories to account for an immunizing effect that is caused by the experience of control per se rather than by the prior strengthening of the test response. It is difficult to argue that learning to turn a small wheel with the paws in a wheel-turn box directly strengthens running in a shuttle box. This wheel-turn escape training should not have an immunizing effect, but it does.

3. If inescapable shock interferes with subsequent escape acquisition because it has led to the development of a motor response incompatible with the escape response, then the explicit reinforcement of such a response should duplicate the effects of exposure to inescapable shock. As already noted, Bracewell and Black (1974) found that the explicit punishment of movement led to a small decrement in subsequent acquisition in a shuttle box. However, this experiment is difficult to interpret for the reasons already noted (see pp. 21–22). Another study using this strategy was reported by Maier (1970). One group of dogs was trained to escape shock by holding still, a response directly incompatible with jumping over a hurdle in a shuttle box. The dogs were restrained in a hammock and panels were placed $\frac{1}{4}$ in. (.64 cm) above and $\frac{1}{4}$ in. (.64 cm) to each side of the dog's head. The dogs could not prevent the onset of shock, but they could terminate shock by *not touching* any of the panels for a specified period of time once the shock came on. Since shock elicits head movements in restrained dogs, the dogs were required to actively inhibit movements elicited by shock. Because the panels were positioned $\frac{1}{4}$ in. (.64 cm) from the dog's head, the dogs could refrain from touching the panels only by remaining almost perfectly motionless, by freezing. A second group of dogs received inescapable shock yoked to the first group and a third received no pretreatment. Following this treatment all groups received 12 days of testing in a shuttle box.

If inescapable shock produces a learned helplessness effect because it induces freezing or leads to the punishment of movement, then the group taught to inhibit movement as

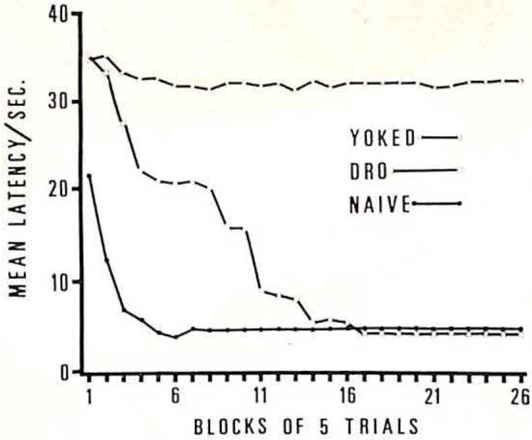

FIGURE 8. Mean latency to respond in a shuttle box for dogs previously given training to escape shock by holding still (DRO), given yoked inescapable shocks, or given no shocks. (The yoked group did not learn, whereas the group taught to hold still did learn.) (From "Failure to Escape Traumatic Shock: Incompatible Skeletal Motor Responses or Learned Helplessness?" by Steven F. Maier, *Learning and Motivation*, 1970, *1*, 157–170. Copyright 1970 by Academic Press. Reprinted by permission.)

a means of escaping shock should later escape at least as poorly as the group given inescapable shock. The learned helplessness hypothesis makes a different prediction. Even though the dogs have learned an incompatible motor response, they have also learned that they have control over shock; and even if they should be slow to learn to escape in the shuttle box, they should eventually learn. In contrast, dogs given inescapable shocks should not learn to escape in the new situation; experiences of failure to escape on the early trials in the new situation should strengthen the assumed cause of interference.

The results of this experiment can be seen in Figure 8. The group (labeled DRO) trained to perform a response incompatible with shuttling was slow to learn shuttling, but all subjects in this group eventually learned. In contrast, half of the subjects given inescapable shock entirely failed to learn across the 12 days of testing. Thus the training of a response demonstrably incompatible with shuttling (negative transfer was produced) did not duplicate the effects of inescapable shock, and this is inconsistent with competing motor response explanations of the learned helplessness effect. It could, of course, be argued that exposure to inescapable shock establishes a response that is stronger or even more incompatible with shuttling than the one trained in the present experiment; the burden of proof, however, would clearly be shifted onto such an argument.

4. It will be recalled that Bracewell and Black (1974) argued that movement occurs during inescapable shock and is punished, presumably because it hurts to struggle. Alternatively, Bracewell and Black suggested that the subject might find some other motor response that reduces the pain produced by shock. This position predicts that if the subject is not allowed to move during exposure to inescapable shock, no learned helplessness should result. If movement does not occur it cannot produce an increment in pain and so be punished, and if movement does not occur the subject cannot find a motor response that mitigates shock.

Overmier and Seligman (1967) prevented movement during exposure to inescapable shock by the administration of curare. Curarization during exposure to inescapable shock did not prevent or even reduce the subsequent learned helplessness effect. It seems to us that this experiment is a direct disconfirmation of the Bracewell and Black (1974) hypothesis.[5] However, Bracewell and Black have argued that this curare experiment does not necessarily contradict their position. Their argument is that movement-related neural processes might be adventitiously reinforced or punished during curarization: "A possibility exists, therefore, that the onset and termination of shock could act inadvertently to reinforce superstitious movement related neural responses in curarized dogs" (Bracewell & Black, 1974, p. 55). It should be carefully noted that this is different from their original position. The Bracewell and Black hypothesis argued that an incompatible response is acquired because it produces a positive outcome of reduced pain or discomfort. Thus Bracewell and Black provided a mechanism whereby the incompatible response is acquired, an *explicit* reinforcement mechanism. But in the case of the curarized subject Bracewell and Black do *not* argue that "the movement related neural processes" reduce pain and are therefore explicitly reinforced since there is no obvious way that such processes could reduce pain. Thus Bracewell and Black argue that in the case of curarized subjects movement-related neural processes are *adventitiously* rather than explicitly reinforced, and this seems gratuitous. Although there is evidence that movement-related neural processes can be explicitly punished (Black, Young, & Batenchuk, 1970), there is no evidence that they can be adventitiously or superstitiously reinforced. Further, even if such processes could be adventitiously reinforced or punished there is no reason to assume that the overt movement produced by such a process would be incompatible with shuttling. It could just as easily facilitate shuttling. To assume that it will be incompatible is, again, gratuitous.

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[5] It should be noted that this experiment has no implications for the Anisman and Waller (1973) competing motor response theory. There is no reason to believe that curarization during inescapable shock should reduce any subsequent freezing.

5. We have already described an experiment by Rosellini and Seligman (1975) (see p. 10) in which rats were given escapable shock, inescapable shock, or no shock, and were then trained to traverse a runway for food. Following training, the rats were extinguished and then allowed to escape from the presumably frustrating goal box. The result was that the rats previously exposed to inescapable shock were slow to escape the goal box but did not differ from controls in either acquisition or extinction. If a motor response is established during exposure to inescapable shock, it is difficult to see why it would interfere with escape from the goal box but not interfere with runway acquisition and extinction.

6. The incompatible motor response theories have difficulty dealing with situations that do not employ electric shocks or other noxious unconditioned stimuli. The particular competing response hypotheses offered by Bracewell and Black (1974) and by Anisman and Waller (1973) cannot account for appetitive analogues to the learned helplessness effect (see p. 12) in animals or learned helplessness effects in human beings induced by exposure to loud noises (see p. 9). Moreover, it is difficult to see how *any* incompatible motor response view could account for the noise-escape failure produced by unsolvable discrimination problems in human subjects (see p. 11). It should be noted that the incompatible motor response hypotheses which we have discussed were designed to account only for the effects of inescapable shock in animals, and it could be argued that these other effects are different phenomena and therefore need not all be explained by the same theory. Such a move seems ad hoc and our preference is for that theory which is able to integrate the largest amount of data.

Motor Activation Deficit Hypothesis

A somewhat different motor response explanation of the learned helplessness phenomenon has been offered by Weiss et al., (1975). Their explanation of the learned helplessness effect is as follows:

> When animals were exposed to severe inescapable shock, a deficiency in central noradrenergic activity occurred, deriving in part at least, from the depletion in the level of norepinephrine seen in this condition. As a consequence of this noradrenergic deficiency the animals could mediate only a limited amount of motor activity, an amount insufficient for learning and performance of the correct response in the shuttle avoidance task on which they were tested. On the other hand, animals exposed to the same shocks but able to control them did not develop this noradrenergic deficiency and therefore could mediate sufficient motor behavior to perform adequately. (p. 7)

A number of factors led Weiss et al. (1975) to develop this explanation of the learned helplessness effect. First, Miller and Weiss (1969) argued that the dissipation of the learned helplessness effect over a 48-hour interval following one session of exposure to inescapable shock in dogs suggests mediation by a time-dependent physiological change rather than by learning. They argued that things that are learned do not disappear across an interval as short as 48 hours. Second, it is known that central catecholamines have a time course of recovery following depletion (e.g., Rech, Bovys, & Moore, 1966) and are involved in the mediation of movement (e.g., Herman, 1970). Third, Weiss (1968, 1971a, 1971b, 1971c) has shown that rats given inescapable shock undergo greater stress than do rats given equivalent escapable shocks, as measured by gastric lesions, loss of body weight, plasma steroid levels, and fearfulness. Finally, this led Weiss, Stone, and Harrell (1970) to assay whole brain norepinephrine following exposure to escapable or yoked inescapable shock. They found norepinephrine levels to be lower following exposure to inescapable shock. This finding has been replicated in separate brain regions by Weiss, Pohorecky, Dorros, Williams, Emmel, Whittlesey, and Case (Note 6) and has been shown to occur regardless of the amount of motor activity required to escape and avoid shock (Weiss, Pohorecky, Emmel, & Miller, cited in Weiss et al., 1975). Following these demonstrations of effect on endogenous norepinephrine levels, Weiss, Pohorecky, McMenima, Berkeley, and Jaffe (cited in Weiss et al., 1975) estimated norepinephrine reuptake and release in vivo by infusion of radioactive (3H) norepinephrine. They found that reuptake occurred at a higher rate in rats previously exposed to inescapable than escapable shocks. This reuptake difference could account for the differences in the level of endogenous norepinephrine.

Weiss et al. (1975) present 12 experiments in support of the notion that the learned helplessness effect occurs because inescapable shock is a severe stressor, produces a temporary depletion of norepinephrine, and thus depresses the movement the organism can emit below what is required for shuttle box acquisition. We will describe each of these 12 experiments. In the first three experiments, Weiss et al. attempted to show that a procedure which deplete norepinephrine but does not involve the factors stressed by the learned helplessness hypothesis duplicates the learned helplessness phenomenon. It is known that a forced swim in 2 °C water rapidly depletes brain norepinephrine levels and reduces its release (Stone, 1970a, 1970b). Thus the motor activation deficit hypothesis would expect a forced swim in cold water to produce a learned helplessness effect. However, a forced swim in warm water (28 °C) does not produce a depletion of brain norepinephrine and so the motor activation deficit hypothesis would not expect a warm swim to produce a learned helplessness effect. Weiss et al. argued that animals forced to swim in either warm or cold water should experience the same lack of control, and that the learned helplessness hypothesis would thus expect similar effects resulting from a warm or a cold swim. In Experiment 1, rats were put in either warm (28 °C) or cold (2 °C) water for either 3.5 or 6.5 minutes. Thirty minutes later the rats were all given an FR-1 shuttle box shock escape/avoidance acquisition test. The results were that the rats exposed to cold swim were severely retarded in shuttle box acquisition, while the warm-swim subjects showed normal acquisition.

Experiment 1 led Weiss et al. (1975) to conclude that cold swim duplicated the failure-to-learn aspect of the learned helplessness phenomenon. Another feature of the learned helplessness phenomenon is that prior training to escape in the shuttle box eliminates the interfering effects of subsequent exposure to inescapable shock. In Experiment 2 Weiss et al. gave rats 25 trials of FR-1 shuttle box training 24 hours before a 3.5 minute cold swim. The rats were again tested in the shuttle box 30 minutes after the cold swim. The pretraining in the shuttle box reduced the deficit produced by cold swim. Another feature of the learned helplessness phenomenon is that it has a time course in dogs following a single session of inescapable shock. In Experiment 3 Weiss et al. gave rats a cold swim and then tested them in a shuttle box 30 minutes, 2 hours, or 48 hours later. With a 48-hour delay, there was no longer any detectable effect of the cold swim. Further, rats tested at the 30-minute point were retested 48 hours later. A deficit was still present upon retest, as was the case with learned helplessness. These three experiments led Weiss et al. to conclude that cold swim duplicates the effects of inescapable shocks.

The next six experiments employed a different strategy. In these experiments, Weiss et al. attempted to find situations in which the motor deficit and the learned helplessness hypotheses make different or opposed predictions. The motor activation deficit hypothesis implies that the magnitude and the likelihood of finding a failure to learn to escape depends on the amount of motor activity required to escape. Weiss et al. claimed that the learned helplessness hypothesis would not expect the amount of effort required to escape to be a potent variable. Experiment 4 by Weiss et al. demonstrated that the detrimental effects of cold swim are strengthened by increasing the height of the barrier in the shuttle box, and Experiment 5 showed that cold swim did not produce a deficit in learning to escape shock when the escape response only required restrained rats to poke their nose through a hole in the front of the tube in which they were confined. In Experiment 6 Weiss et al. attempted to demonstrate that exposure to inescapable shock interferes with acquisition of escape behavior in their FR-1 test task. They mentioned that they have had difficulty in producing such an effect, but showed that a 50-min. exposure to 4.0 mA 2.0-sec shocks occurring every 20 sec produces an escape deficit when the rat is tested 30 min later. Experiment 7 showed that such shocks interfered with shuttle box but not with nose-poke acquisition, as predicted by the motor deficit hypothesis.

The next three experiments by Weiss et al. are based on data (Zigmond & Harvey, 1970) which indicate that the central noradrenergic response to stress habituates with repeated exposure to stress. That is, the degree of norepinephrine depletion produced by a stressor is much reduced if the organism has been repeatedly stressed previously. This leads the motor activation deficit hypothesis to predict that repeated exposures to the stressful condition should *reduce* any subsequent deficit in a test task. The learned helplessness hypothesis surely does not make such a prediction. In Experiments 8 and 9 Weiss et al. found that 14 sessions of exposure to cold swim or inescapable shock eliminated the FR-1 shuttle box escape deficit produced by cold swim or inescapable shock. In addition, these experiments confirmed the finding that norepinephrine is not depleted after repeated stress.

A variety of drug treatments were explored by Weiss et al. in the final three experiments. Experiment 10 found that tetrabenazine, a relatively nonspecific depletor of monoamines, produced poor performance in a shuttle box 30 minutes after administration. Experiment 11 showed that repeated administration of tetrabenazine did not affect shuttle box performance. Finally, Experiment 12 found that pargyline, a drug which inhibits monoamine oxidase and therefore prevents intraneuronal degradation of monoamines, prevented inescapable shock from affecting FR-1 shuttle box performance.

Discussion of the motor activation deficit hypothesis. We have presented the entire logical and evidential bases of the motor deficit hypothesis without comment or interruption because it presents, at least on the surface, a convincing case and should be seen as such by the reader. However, we feel that every step in the argument which suggested the motor deficit hypothesis and each piece of data which seems to support the hypothesis is questionable and open to alternative interpretation, and we will discuss each, point by point. Following this discussion we will present a variety of lines of evidence which we feel are inconsistent with the motor deficit hypothesis.

It will be recalled that a physiological depletion hypothesis was first suggested by Miller and Weiss (1969), who argued that the time course of the learned helplessness effect is not consistent with the notion that something learned during exposure to inescapable shock is the cause of the effect. They argued that if something is learned it should still be present 48 hours later. Although this point is not crucial to the motor deficit hypothesis, it should be pointed out that it is highly debatable. Proactive and retroactive interference produce memory losses in animals (Maier, Allaway, & Gleitman, 1967; Maier & Gleitman, 1967) as well as man, and both proactive and retroactive interference effects generally increase with time since learning. It is possible that prior experiences with learning to control events could interfere with the retention of the learning that shocks are uncontrollable after only one sesison with inescapable shocks. In addition, there is ample evidence in animals for retention losses over short intervals of time (see D'Amato, 1973; Spear, 1973).

More importantly, Weiss et al. (1975) ignored aspects of the learned helplessness time course data which are not as congenial to their hypothesis as is the fact that the effect does not occur after 48 hours following a single experience with inescapable shocks. Seligman and Groves (1970) have shown that the time course of the learned helplessness effect is eliminated in dogs if the dogs are given four sessions rather than one session of inescapable shock. Such dogs failed to learn to escape even when tested 7 days after their last exposure to inescapable shock. Further, if dogs are raised in laboratory cages and thus deprived of extensive experience controlling events, only two sessions of inescapable shock are required to produce nontransient learned helplessness effects (Seligman & Groves, 1970). It should be noted that these are precisely the results that would be expected from a memory interpretation of the time course and are not explicable by the motor deficit hypothesis in any obvious way. Norepinephrine levels should certainly be recovered by 7 days following exposure to inescapable shock. Finally, Seligman and Beagley (1975) failed to find a time course of learned helplessness with rats as subjects after only one session of inescapable shock. Rats given inescapable shock failed to learn to escape when tested 7 days after exposure to a single session of inescapable shock. This finding is particularly damaging to the motor deficit hypothesis because this hypothesis rests on a base of data obtained with rats, not dogs, as subjects. It would be interesting if rats raised in a natural environment, rather than in laboratory cages, showed a transient helplessness effect. In summary, the transient effect required by the motor deficit hypothesis occurs only under one condition—a single session of inescapable shock with mongrel dogs not raised in the laboratory. The time course data taken as a whole does not invalidate a learning interpretation and may be more consistent with the learned helplessness hypothesis than with the motor activation deficit hypothesis.

Of greater importance in establishing the plausibility of the motor activation deficit hypothesis are the several demonstrations by Weiss and his colleagues that exposure to inescapable shock is highly stressful and reduces endogenous norepinephrine levels while exposure to escapable shock is less stressful and does not lead to such a reduction. It is this that allows the motor deficit hypothesis to explain why the learned helplessness effect occurs and is determined by the controllability of the initial shock. Several comments are in order. Weiss et al. (1975) argue that the learned helplessness effects which we have found are caused by norepinephrine depletion produced by the inescapable shock which *we* deliver. Therefore, it is *required* that Weiss and his colleagues demonstrate that *our* inescapable shock conditions produce intense stress and norepinephrine depletion. It is thus crucial to inquire whether the experiments by Weiss and his colleagues employed conditions roughly similar to ours. Table 3 summarizes some of the relevant aspects of our rat helplessness experiments and the Weiss studies. As can be seen, the behavioral helplessness studies use 1.0-mA shocks with session lengths lasting from 1 to 1½ hours. The rat's behavior is tested 24 hours or longer after the inescapable shock session. The conditions used by Weiss and his colleagues *bear no resemblance at all* to these conditions. All but one of the norepinephrine studies exposed the subjects to a *minimum of 20 hours* of inescapable shock, and shock intensity was a *minimum of 3.0 mA*. Further, norepinephrine was typically assayed *immediately* following the end of the inescapable shock session. It is worth noting that the one norepinephrine study which deviated from these parameters, even though still very different from our parameters (Weiss et al., 1970, Experiment 1), did *not* find depletion of norepinephrine in the group given inescapable shocks. This experiment

TABLE 3

SUMMARY OF THE PARAMETERS USED IN THE RAT LEARNED HELPLESSNESS EXPERIMENTS

Experiment	Maximum shock level	Session length	Time between inescapable shock and test
Behavioral helplessness			
Maier, Albin, & Testa	1.0 mA	1½ hr.	24 hr.
Goeckner, Greenough, & Maier	1.0 mA	1 hr.	24 hr.
Testa, Juraska, & Maier	1.0 mA	1½ hr.	24 hr.
Maier & Testa	1.0 mA	1½ hr.	24 hr.
Seligman & Beagley	1.0 mA	1½ hr.	24 hr.
Seligman, Rosellini, & Kozak	1.0 mA	1½ hr.	5 min.; 1 hr.; 24 hr.; 168 hr.
Seligman, Hannum, & Rosellini	1.0 mA	1½ hr.	65 days
Ulcers, steroids			
Weiss (1968), Experiment 2	1.6 mA	21 hr.	12 hr.
Weiss (1971a)	4.0 mA	48 hr.	none
Weiss (1971b)	4.0 mA	48 hr.	none
Weiss (1971c)	4.0 mA	48 hr.	none
Norepinephrine			
Weiss, Stone & Harrell, Experiment 1	3.0 mA	3 hr.	20 min.; 40 min.
Weiss, Pohorecky, Dorros, Williams, Emmel, Whittlesey, & Case	3.4 mA	20 hr.	none
Weiss, Pohorecky, Emmel, & Miller	not known	24 hr.	none
Weiss, Pohorecky, Emmel, McMenima, Berkeley, & Jaffe	not known	20 hr.	2 min.

found a reliable overall difference in norepinephrine levels between inescapable, escapable, and no-shock groups. However, the inescapable group did not differ reliably from the no-shock group, and the overall difference was due to an *elevation* of norepinephrine in the escapable shock group.

It should be clear that, in order to explain the learned helplessness effect, the motor activation deficit hypothesis requires that a 1-hour session employing 1.0-mA shocks (occurring about one per minute) produce a depletion in norepinephrine which is still present 24 hours following the termination of the inescapable shock session. There is not a shred of evidence for such an effect. It is a curious argument that holds a behavioral deficit which occurs 24 hours after a 1-hour session with 1.0-mA shocks is attributable to norepinephrine depletion because norepinephrine levels are low when measured immediately after massive 20-hour sessions with 3.4- or 4.0-mA shocks.

At this point it should be noted that Weiss et al. (1975) have restricted their explanation to only the *dog* helplessness experiments, and not the rat helplessness experiments. However, all of the data obtained by Weiss and his colleagues came from rats, and so we feel that a comparison of their parameters with those used in our rat studies has been proper. How can one argue that data obtained from rats apply to data obtained from dogs but not to parallel data obtained from rats? Further, the parameters used in our dog studies also differ massively from those used by Weiss et al.—we have typically used 64 shocks spread over about 1½ hours and have tested the dogs 24 hours later.

Moreover, Weiss et al. (1975) have reported that endogenous levels of norepinephrine are depleted in rats given inescapable shock *only if the rats were group housed prior to the experiment*. That is, in all of the neurochemical studies referred to above, the rats were group housed before treatment. If the rats were individually housed before treatment inescapable shock did not deplete endogenous norepinephrine levels. This may be because individual housing in rats induces increased activity of tyrosine hydroxylase (Segal, Knapp, Kuczenski, & Mandell, 1973), a rate-limiting enzyme in the synthesis of norepinephrine. It should be noted that the rats in all of our experiments have been *individually housed* prior to treatment. In fact, Goeckner, Greenough, and Maier (1974) manipulated housing conditions from weaning to adulthood and found that inescapable shock produced a learned helplessness effect whether the rats were group housed or individually housed.

We conclude that the neurochemical data said to provide plausibility to the motor activation deficit explanation of learned helplessness, in fact, provide little if any support.

We now turn to a consideration of the 12 experiments reported by Weiss et al. (1975) as supporting the motor deficit interpretation of the learned helplessness effect. Recall that the first three experiments were designed to show two things: (a) that a treatment which depletes norepinephrine (cold swim) but cannot be said to lead to the learning that aversive events and responding are independent produces all of the characteristics of the learned helplessness phenomenon; (b) that a procedure which involves the same degree of uncontrollability as the first but does not deplete norepinephrine (warm swim) does not produce a learned helplessness effect. We do not feel that the experiments actually demonstrate that which is claimed.

Recall that Weiss et al. compared the effects of a cold swim and a warm swim on FR-1 shuttle box acquisition and argued that the learned helplessness hypothesis would expect equivalent effects from these two treatments since they involve equal uncontrollability. This is not correct. The learned helplessness hypothesis does not argue that failure to learn to escape shock results from exposure to sheer uncontrollability, but by exposure to uncontrollable *aversive* events. For example, the learned helplessness hypothesis would not expect failure to escape shock to result from exposure to a light presented independently of behavior. Cold swim and warm swim are certainly not equally aversive, and warm swim may not be aversive at all. Further, although we do not wish to argue in this direction, it is not clear to us why cold swim cannot be described as a condition in which escape from the highly aversive freezing water is independent of behavior. Cold swim produces muscular debilitation while warm swim does not. Thus the trauma in a cold swim may indeed be more uncontrollable than in a warm swim.

More important than whether the learned helplessness hypothesis predicts equivalent effects of cold and warm swim, we do not believe that Weiss et al. (1975) have demonstrated that cold swim produces a behavioral deficit that is the same as the learned helplessness effect. Learned helplessness is defined as an effect resulting from the uncontrollability of aversive events. As noted previously, FR-1 shuttle box acquisition may not be sensitive to such effects in rats, and a variety of experimenters have failed to find any effect of inescapable shock on subsequent FR-1 shuttle box acquisition. There are a few experiments which find an effect of inescapable shock on subsequent FR-1 shuttle box acquisition (e.g., Weiss et al., 1975, Experiment 6) but these experiments omit comparison of the effects of equivalent escapable and inescapable shock so it cannot be concluded that a learned helplessness effect with FR-1 shuttling has been demonstrated. Since Weiss et al. employed FR-1 shuttling as their dependent variable, they may have been using a test that is not sensitive to learned helplessness effects.

Further, Weiss et al. (1975) used a 30-minute interval between cold swim and the behavioral task, whereas our learned helplessness experiments use a 24-hour interval between inescapable shock exposure and test. Thus Weiss et al. must demonstrate that cold swim produces a deficit 24 hours later, not 30 minutes later. Experiment 3 of Weiss et al. examined the time course of the cold-swim effect, but it does not help us here because Weiss et al. did not have a 24-hour time point. Their experiment only involved testing at 30 minutes, 2 hours, or 48 hours. An examination of the Weiss et al.

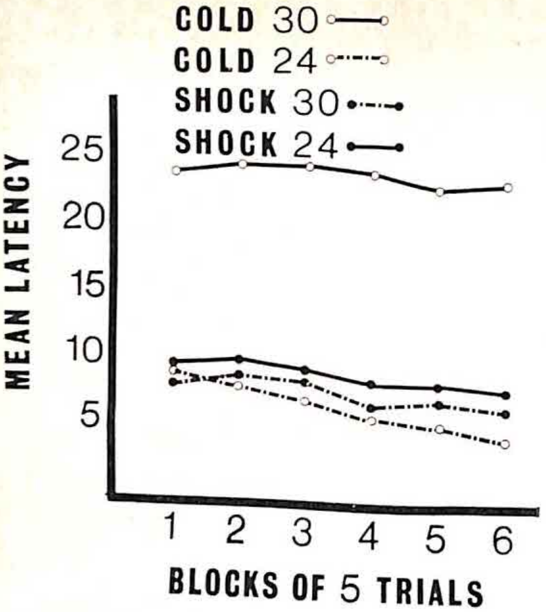

FIG. 9. Mean shuttle box FR-1 escape response latency for rats given either cold swim or inescapable shocks 3 minutes or 24 hours before the shuttle box test (Jackson & Maier, unpublished data 1975). (Only the rats given cold swim 30 minutes before the test failed to respond.)

data reveals that the cold-swim effect was almost absent at 2 hours. However, the 24-hour point is still needed, and Maier has now supplied the missing point. Jackson and Maier (unpublished data) gave rats 3.5 minutes of cold swim in 2 °C water and tested them in a FR-1 shuttle box task 30 minutes or 24 hours later. The results can be seen in Figure 9. Jackson and Maier found the same debilitation as found by Weiss et al. 30 minutes after cold swim, but there was *no deficit at all* if 24 hours had intervened. It should also be noted that the learned helplessness effect in rats, the effects of exposure to uncontrollable shock on subsequent escape acquisition in a task sensitive to controllability, may not show any dissipation with time.

Another aspect of Experiment 3 is worthy of comment. Weiss et al. (1975) note that dogs that were tested 24 hours after exposure to inescapable shock and that failed to learn to escape, would fail to learn again if retested later. That is, the failure became permanent. Weiss et al. retested the rats that failed to learn to escape 30 minutes following cold swim after an additional 48 hours. A deficit was still present and Weiss et al. concluded that this duplicates the corresponding characteristic of the learned helplessness phenomenon in dogs. We do not think so. In the dog experiments, there was no improvement at all during retest; the dogs showed no tendency to learn. This was not true of the rats in the Weiss et al. study. There were 25 trials on the retest, and by the last block of 5 trials the response latency of the cold swim rats was down to about 7 sec, as compared with about 4 sec in a control group. In fact, in a later experiment (Experiment 7) the retest effect disappeared completely, although Weiss et al. did not comment on this aspect of the data.

The final experiment (Experiment 2 in Weiss et al., 1975) said to reveal the identity of the cold swim and learned helplessness effects is the immunization effect. It is worth noting that although Weiss et al. found prior training in the shuttle box to reduce the effects of cold swim on shuttle box performance, the reduction was not complete; some debilitation still occurred. The immunizing effect on learned helplessness is *complete* in both dogs (Seligman & Maier, 1967) and rats (Seligman et al., 1975) when the immunization treatment is given in the shuttle box.

We conclude that the experiments provided by Weiss et al. (1975) do not demonstrate that the behavioral deficit produced by cold swim is the same as the deficit produced by the uncontrollable shocks used in the learned helplessness experiments. Even more convincing to us than the discrepancy between the Weiss et al. and the learned helplessness experiments are our observations of subjects 30 minutes after cold swim, the point at which Weiss et al. tested such subjects. A severe motor debilitation *does* appear to be present. The rats have great difficulty moving and simply remain motionless when placed on a table. Further, such rats have some difficulty righting themselves when placed on their sides. When they are removed from the 2 °C water they are completely limp. Stone (1970b) has extensively studied the effects of cold swim. Observing the rats after a 20-minute swim in 14.5 °C water, Stone notes:

> Shortly after the stress, the swim stressed rats appear exhausted; they lie in a prostrate position, are unable to move their rear limbs, and have difficulty righting themselves when laid on one side. During the later stages of recovery, the animals remain immobile in a crouched position and show ptosis with continued and violent shivering. (p. 53)

We have observed similar behavior after 3.5 minutes in 2 °C water.

The inescapable shocks used in our learned helplessness experiments do not produce anything like these obvious signs of motor debilitation. As noted previously, the shocks which we use have no effect on runway acquisition 24 hours later, nor do they affect FR-1 shuttle box performance. We doubt that Weiss's subjects could even locomote to the end of a runway 30 minutes after cold swim. Thus we conclude that Weiss et al. (1975) are indeed correct in arguing that cold swim produces a motor deficit, perhaps via norepinephrine depletion. However, there is no reason to believe that the levels of inescapable shock used in the learned helplessness dog or rat experiments produce a motor deficit 24 hours later.

The factors just discussed are also important in interpreting the subsequent experiments by Weiss et al. in which the motor activation deficit hypothesis and the learned helplessness hypothesis were said to make opposed predictions. Experiments 4 and 5 demonstrated that the amount of motor activity required to perform the task response given 30 minutes after cold swim determined whether a deficit occurred, and thus supported the notion that cold swim results in a temporary motor deficit. These experiments do support this notion, but there is nothing in the learned helplessness position which denies that a cold swim produces a motor deficit. The learned helplessness position does not predict this, but there is no reason why it should.

Experiments 6 and 7 of Weiss et al. (1975) use electric shocks during pretreatment, but 150 4.0-mA shocks were delivered whereas the learned helplessness experiments with rats used 64 to 80 1.0-mA shocks. The level of shock used by Weiss et al. is almost of tetanizing intensity and may indeed lead to a trauma-induced motor deficit 30 minutes later. Our dog experiments used 64 6-mA shocks, and testing occurred 24 hours later. It is difficult to compare shock intensities across species as different as dogs and rats, but our observations indicate 4 mA in rats to be much more severe than 6 mA in dogs. Jackson and Maier (unpublished data) investigated whether exposure to shocks characteristic of learned helplessness experiments would produce the sort of deficit reported by Weiss et al. Rats were exposed to 64 1.0-mA shocks and tested in an FR-1 shuttle box task either 30 minutes or 24 hours later. As can be seen in Figure 10, these shocks did not produce a deficit even 30 minutes later. Thus Experiments 6 and 7 employed conditions very different from those employed in the learned helplessness experiments with rats and may indeed be correctly interpreted in terms of a motor deficit. The fact that 150 4.0-mA shocks produce a motor deficit 30 minutes later does not imply that 64 1.0-mA shocks produce a motor deficit 24 hours later.

The conceptual base for Experiments 8 and 9 may indeed be adequate for a test between the motor activation deficit and learned helplessness explanations of the learned helplessness phenomenon. Weiss et al. noted that the reduction in norepinephrine levels produced by exposure to stressors habituates with repeated exposure to the stressor. Thus if inescapable shock produces a learned helplessness effect because it produces a motor deficit through norepinephrine depletion, repeated exposure to inescapable shock should not produce a learned helplessness effect. The learned helplessness hypothesis certainly does not make this prediction. It will be recalled that Weiss et al. found that rats repeatedly exposed to cold swim or to 4.0-mA shocks did indeed perform well when tested on FR-1 shuttling. On the surface this result seems quite deci-

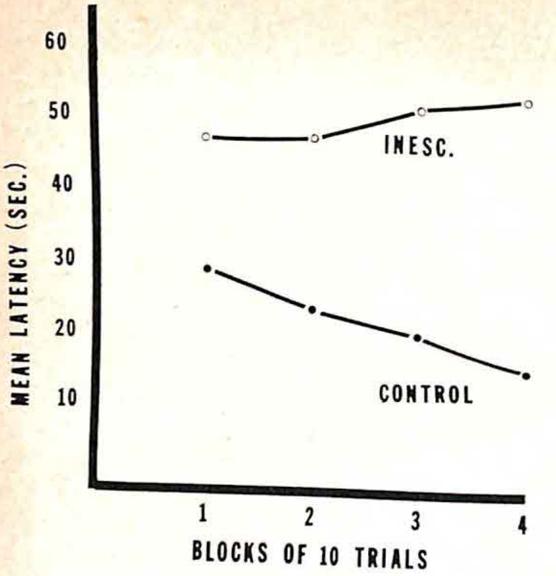

FIGURE 10. Mean FR-3 lever press latencies for rats given 15 days of inescapable shocks or no shock (Seligman & Rosellini, unpublished data). (The inescapable shock group did not learn.)

sive. However, it should be recognized that the Weiss et al. logic demands that repeated exposure to the *same shock conditions* as employed in learned helplessness experiments prevents a deficit on a task *known to be sensitive* to the degree of control which organisms have over initial shock experience.

Thus we have each independently repeated the Weiss et al. (1975) experiment with the shock parameters normally used in our laboratories and with test tasks known to be sensitive to learned helplessness of controllability effects. Rosellini and Seligman (unpublished data) gave one group 15 daily sessions of 1.0-mA inescapable shocks. A second group received no treatment. All three groups were then tested in an FR-3 level pressing task. Maier employed 10 days of exposure to either escapable, inescapable, or no shocks in a wheel-turn apparatus and then tested his subjects in an FR-2 shuttling task. The shock level was 1.0 mA. The results of these experiments can be seen in Figures 10 and 11. It is clear that repeated exposure to 1.0-mA shocks does not reduce the deficit seen in tasks known to be sensitive to learned helplessness effects.

Many of the comments made thus far are also appropriate to Experiments 10 through 12. Recall that in these experiments Weiss et al. (1975) found that pharmacological depletion of monoamines produced poor FR-1 shuttle box performance 30 minutes later, and that pharmacological blockade of monoamine degradation prevented the effects of inescapable shock 30 minutes later in a FR-1 shuttle box task. This is consistent with the notion that Weiss et al. are studying a motor debilitation mediated by monoamines, but as already noted, this may be a very different phenomenon from learned helplessness.

We conclude that the experiments presented by Weiss et al. do not strongly support the motor activation deficit hypothesis as an explanation of the learned helplessness effect. It should be stressed that we are *not* arguing that the learned helplessness hypothesis can account for the data presented by Weiss et al. We think it likely that Weiss et al. were quite correct in their interpretation of their own data. Weiss et al. have demonstrated a number of very interesting effects, but they may not be

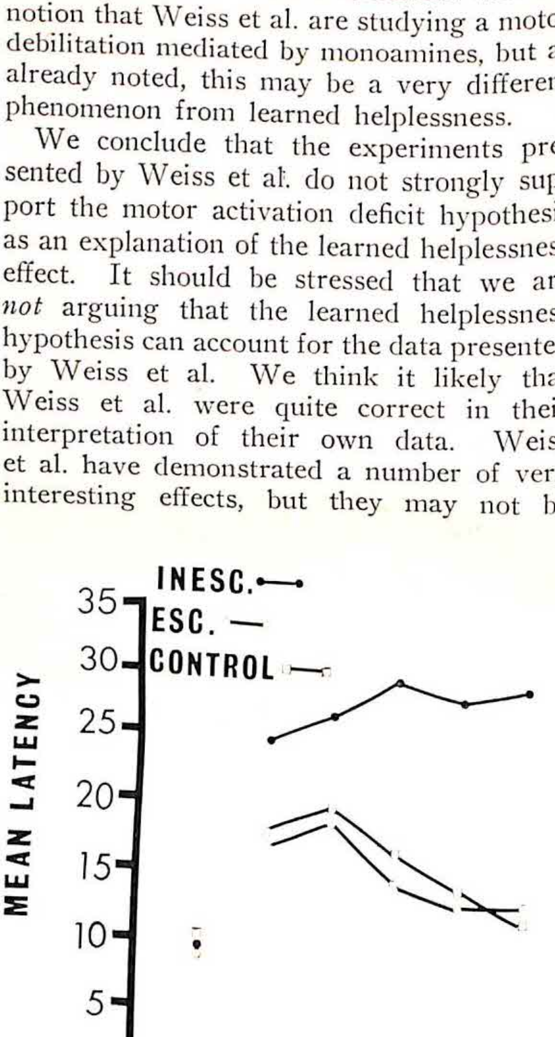

FIGURE 11. Mean FR-2 shuttle box response latencies for rats given 10 days of escapable, yoked inescapable, or no shocks (Maier, unpublished data, 1975). (The inescapable shock subjects failed to learn.)

closely related to learned helplessness effects. The basic similarity is that in both cases animals failed to escape shock, but there may be many roots to producing such a deficit, and they may not reduce to one explanation. The deficit produced by exposure to extremely traumatic events as used by Weiss et al. may be produced by a very different mechanism from the deficits produced by exposure to much less traumatic *uncontrollable* aversive events in the learned helplessness experiments.

Other evidence. There are a number of findings not considered by Weiss et al. which are difficult for the motor activation deficit hypothesis to explain. Many of these have already been described and only a brief discussion is necessary.

1. We have already described experiments by Maier and Testa (1975) which indicate that although inescapably shocked rats are poor at learning FR-2 shuttling, they will learn as well as controls when there is a brief break in shock after the first response of the FR-2. In an analogous fashion, inescapably shocked rats readily learned FR-1 shuttling when shock terminated immediately after the response, but failed to learn when shock termination was briefly delayed. The motor activation deficit hypothesis cannot explain why the behavior of inescapably shocked subjects is so strongly controlled by the nature of the contingency between responding and shock termination in the test task. The same amount of motor activity is required to perform an FR-2 response whether or not there is a brief break in shock, but the outcomes are very different. Similarly, the amount of motor activity required to make a single crossing of a shuttle box is the same whether or not shock termination is slightly delayed, but the outcome is dependent on this factor. Again, mention should be made that Weiss et al. restricted their explanation to learned helplessness in the dog, but this is ad hoc and it does not seem justifiable to restrict consideration to only a small part of the data.

2. Recall that Seligman and Maier (1967) found that prior experience with controllable shock in a shuttle box eliminated the interfering effects of subsequent inescapable shocks on shuttle box performance. The motor activation deficit hypothesis is able to account for this immunizing effect by arguing that pretraining in the shuttle box reduces the amount of motor behavior required during testing because the subject does not have to search for the correct response—it has already been trained. We have presented new data (p. 26) indicating that prior experience with controllable shock has an immunizing effect even when such prior experience is given with a different response from that later used in testing. The motor activation deficit hypothesis cannot explain this effect. Prior training to turn a wheel with the paws should not reduce the amount of motor activity required to find the correct response in a shuttle box.

3. Seligman, Maier, and Geer (1968) and Seligman and Beagley (1975) found that escape deficits in dogs and rats already failing to escape could be eliminated by forcible exposure to the escape contingency. It is hard to see how the motor deficit hypothesis can account for this therapy effect. Does forcible exposure to the escape contingency produce a rapid recovery of norepinephrine levels? It is interesting to note the Abramson and Seligman (unpublished data) have found that forcible exposure to the escape contingency eliminates the behavioral deficit even in rats whose norepinephrine levels have been depleted by injection of α-Methyl-para-tyrosine.

4. We have several times referred to the fact that the learned helplessness effect does not have a time course in dogs when the dogs are given a number of sessions of inescapable shock (Seligman & Groves, 1970) and may not have a time course in rats at all (Seligman & Beagley, 1975). This is directly opposed to the expectations of the motor deficit hypothesis.

5. Hannum et al. (in press) found that rats given four sessions of inescapable shock shortly after weaning failed to learn to escape as adults. Such a failure did not result if the shocks were escapable. Do four sessions

of inescapable shock shortly after weaning produce a permanent depletion of norepinephrine?

6. Although Weiss et al. (1975) have explicitly limited the score of their hypothesis to learned helplessness experiments employing electric shocks to animals (even, on an ad hoc basis, to dogs), mention should be made that there are a large number of experiments outside this domain which can be integrated by the learned helplessness hypothesis but not by the motor activation deficit hypothesis. Examples are the effects of exposure to inescapable noise and unsolvable problems on problem-solving behavior in humans (see p. 13), and the possible analogues to shock-induced learned helplessness produced by delivery of appetitive events independently of behavior (see p. 12) in animals.

CONCLUSIONS

We feel that the burden of the evidence is that the motor theories here discussed cannot account for the learned helplessness phenomenon. It seems to us that the central shortcoming of these theories is that they are performance theories. That is, they claim that inescapably shocked subjects are later less likely to emit the test task response, not that they are less likely to learn from an exposure to the contingency between responding and relief if they do respond. The learned helplessness hypothesis also argues for a performance deficit (the reduced incentive motivation proposition), but in addition argues that learning will be undermined (the associative interference proposition). We feel that the Maier and Testa (1975) experiments (see p. 24) offer conclusive evidence that exposure to inescapable shock has an effect on associative processes as well as on motivational processes.

This should *not* be taken to mean that we feel that the processes discussed by Bracewell and Black (1974), Anisman and Waller (1973), and Weiss et al. (1975) do not exist and do not influence behavior. Organisms can, of course, learn motor responses in one situation that are able to interfere with the acquisition of motor responses in a different situation. Further, the data presented by Weiss et al. do convincingly point to the conclusion that severe trauma induces a transient motor deficit perhaps mediated by norepinephrine depletion. What we dispute is that these processes are sufficient to account for all or even many of the learned helplessness effects. We also dispute that these hypotheses handle the data better than the learned helplessness hypothesis, and we feel that a stronger case has been presented for the learned helplessness hypothesis.

It should be noted, however, that the learned helplessness hypothesis is not without its problems. We will list those that loom largest.

1. The time course of learned helplessness effects has yet to be satisfactorily explained. Recall that one session of inescapable shock produces only a transient effect in dogs, while four sessions of inescapable shock seem to produce a more permanent effect. Moreover, there are data to indicate that the learned helplessness effect may be permanent in rats even after one session of inescapable shock.

This pattern of results is difficult to explain. The learned helplessness theory could argue (see pp. 30–31) that previous experience controlling events proactively interferes with the memory that shock is inescapable. Proactive interference is known to increase with time from learning and to decrease with additional training, and so the dog effects can be explained. But, why do rats show a permanent effect after one session? A possibility concerns the rearing conditions of our subjects. Our dogs were not laboratory- or cage-reared, while the rats we used were raised in cages. Perhaps this reduced the likelihood that the rats encountered many immunizing experiences with controllable events. Consistent with the argument, Seligman and Groves (1970) found that only two sessions of inescapable shock were required to produce a permanent learned helplessness effect in cage-reared beagles while four sessions were required to produce a permanent effect in mongrels raised outside the laboratory. However, such an explanation of the time course effects

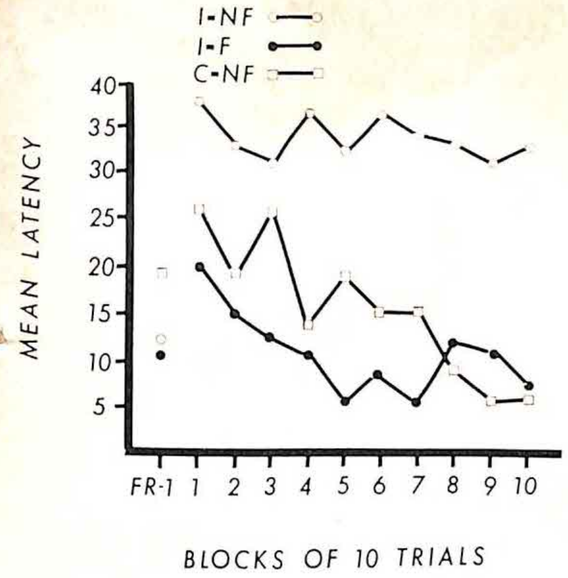

FIG. 12. Mean FR-3 lever press latencies for rats previously given either inescapable (I) or no (C) shocks and then given noise feedback (F) or no feedback (NF) during lever press escape training (Jackson, Tomie, & Maier, Note 7). (Tone feedback eliminated failure to escape.)

is entirely post hoc and is not supported by an independent body of evidence. Additional experimental support will be required before any confidence can be placed in this interpretation.

2. Exposure to inescapable shock produces a deficit on some escape tasks but not on others. Maier et al. (1973) found that exposure to inescapable shock interfered with FR-2 shuttling but not with FR-1 shuttling in rats. Maier et al. were able to account for this difference in terms of the elicited nature of FR-1 shuttling in rats. In an analogous fashion, Seligman and Beagley (1975) found that inescapable shock has no effect on FR-1 lever pressing in rats. However, Seligman and Beagley did not find a differential effect of escapable and inescapable shock on FR-2 lever pressing. A clear learned helplessness effect only emerged with FR-3 lever pressing. It is not obvious why this should be so (Seligman & Beagley, 1975). Further, Weiss et al. (1975) found no effect of inescapable shock on a nose-poke escape response. The Maier et al. explanation does not apply to the Weiss et al. data because a clear acquisition function for nose-poke responding was found.

It might seem that the amount of physical activity involved in these various tasks is a determining factor. However, recall that Maier and Testa (1975) found that inescapably shocked rats learn FR-2 shuttling if a brief break in shock occurs after the first response, and they fail to learn FR-1 shuttling if shock termination is delayed. Here physical activity cannot be a factor. The Maier and Testa experiments suggest that the amount of feedback produced by the response could be a crucial factor. A recent experiment by Jackson, Tomie, and Maier (Note 7) supports this notion. Jackson et al. began by replicating the Seligman FR-3 effect. As shown in Figure 12, rats given inescapable shock were very poor at FR-3 lever press escape responding. Another group of inescapably shocked rats was also tested in the FR-3 lever pressing situation. However, a 100-msec burst of white noise was sounded following each lever press, thus providing feedback. As can be seen in Figure 12, these rats learned rapidly. It should be noted that a warning signal was used in the Weiss et al. (1975) nose-poke experiment. This signal terminated with the response, thus possibly providing feedback. Most interesting is the fact that the only study in the literature (Bracewell & Black, 1974) to find an effect of inescapable shock on FR-1 shuttling in the rat did not use a warning signal. Again, much more work will be needed before any confidence can be placed in such an explanation.

Mention should be made that the helplessness effect has seemed more fragile in rats than in dogs or humans. Both of us have encountered occasional difficulty in producing a reliable phenomenon. The feedback effect discussed previously may help to explain this fragility—we may be using test tasks with a relatively large amount of *intrinsic* feedback.

3. The learned helplessness hypothesis is vague in its specification of boundary conditions. Further, there is little empirical evidence regarding such boundary conditions. Should exposure to loud noise presented in-

dependently of behavior interfere with the acquisition of responses that escape electric shock? Should exposure to uncontrollable shock interfere with the acquisition of responses that procure food? Should exposure to noncontingent food interfere with the acquisition of responses that terminate shock? Should exposure to uncontrollable shock interfere with Pavlovian conditioning? Many further questions of this sort could easily be posed. The learned helplessness hypothesis does not make any predictions regarding the results of such experiments. Further, there are very few experiments directed at answering these questions. The learned helplessness hypothesis will have to become more specific, and experiments designed to delineate these boundary conditions will have to be conducted.

4. A final problem concerns the relationship between the subject's perception of independence between responding and outcome and the conditions which lead to such a perception (see p. 17). In our experiments we have arranged conditions in which the relationship between responding and outcome is objectively independent. However, subjects do not always respond to random relationships as if they were random[6] (e.g., Bruner & Revusky, 1961; Hake & Hyman, 1953; Naylor & Clark, 1968), and it is well known that subjective probabilities do not reflect objective probabilities with accuracy (cf. Kahneman & Tversky, 1972). Thus we will have to specify the conditions under which the perception of independence develops—*only* these conditions should lead to learned helplessness.

Before closing a few comments are in order.

It must seem to the reader that a great deal of theoretical confusion surrounds the learned helplessness phenomenon. A number of investigators have proposed alternative accounts of the same behavioral phenomenon and have presented experimental evidence which they feel supports their position and refutes the learned helplessness hypothesis. We, on the other hand, have questioned the implications of these experiments for the learned helplessness hypothesis. Why all this confusion? We feel that a large part of this confusion stems from an error of inference noted previously. The logical structure of many of the experiments which have been presented as inconsistent with the learned helplessness hypothesis have the following form: (a) A procedure is found which produces an escape deficit, (b) this procedure is hard to interpret in terms of the learning of independence between responding and shock termination, and (c) therefore the learned helplessness hypothesis is contradicted. This seems to us to be poor logic. The learned helplessness hypothesis does not argue that all escape deficits are produced by learning that responding and shock termination are independent. There are undoubtedly many ways to produce poor escape performance and there is no reason to expect that all of these operate through a single mechanism.

Just as there may be many different ways to produce escape-learning deficits, exposure to inescapable shock may itself produce a variety of changes in the organism, not just one. Under some conditions, organisms might learn a motor response, stress might be induced, and the organism might learn that its behavior is independent of shock termination. A complete explanation of the influence of exposure to inescapable aversive events on later behavior might involve a consideration of each of these factors. Thus we do not view the various theoretical positions that have been here discussed as incompatible: They might be complementary. The existence of one of these processes does not imply the nonexistence of the others. We feel that we have presented convincing evidence for the reality of the processes specified by the learned helplessness hypothesis; however, this evidence does not imply that other processes do not also occur.

It might seem that this makes the learned helplessness hypothesis difficult to test, but this is not so. A variety of predictions made by the learned helplessness hypothesis have been tested and confirmed (e.g., Hiroto & Seligman, 1975; Maier, 1970; Maier et al., 1973; Maier & Testa, 1975; Overmier & Seligman, 1967; Seligman & Beagley, 1975; Seligman & Groves, 1970; Seligman & Maier, 1967; Seligman, Maier, & Geer, 1968; Testa, Juraska, & Maier, 1974) Failures of these predictions would have been evidence against the learned helplessness position. In addition there are many new ways in which the hypothesis could be tested. For example the *logic,* if not the execution, underlying Experiments 8 and 9 of Weiss et al. (1975) seems to us to be adequate to test the learned helplessness position. The coming years should see a number of such tests and a more detailed elaboration of the learned helplessness hypothesis.

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[6] Thanks go to Christopher Peterson for these references.

Finally, we would like to say a word about the cognitive nature of our position. The learned helplessness hypothesis has been stated in cognitive languages, whereas most of the alternative views have been stated in S–R language. We have found it difficult to even approach the sort of phenomena that we have tried to explain within an S–R framework, and have found the cognitive theorizing to be more fruitful and to reflect more accurately those processes that we feel to be reflected in behavior. Many other investigators of learning and motivation in lower organisms seem to have reached a similar conclusion (e.g., Bolles, 1972; Irwin, 1971; Kamin, 1969; Seligman & Johnston, 1973; Terry & Wagner, 1975; Wagner, Rudy, & Whitlow, 1973). The next few years should determine the value of such an approach.

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Learned Helplessness: A Reply and an Alternative S–R Interpretation

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At the present stage of development within psychology, a periodic need exists for the introduction of a bold, catalytic viewpoint that challenges the foundation of contemporary thinking by placing stress upon the interpretive boundary conditions of existing models. In addition it should offer an alternative model with integrative and predictive powers. The learned helplessness theory described in the preceding article by Maier and Seligman (1976) fulfills the above objectives. These authors are not only responsible for pioneering new areas of research but have simultaneously challenged, both on empirical and theoretical grounds, the existing infrahuman S–R models and dynamic clinical models of depression. Their extensive review of the literature represents a perceptive critical analysis of their opponents' positions as well as a serious attempt to state the weakness of their own viewpoint. The heuristic value of their position is reflected in the fury of research activity which has been generated over a wide variety of issues. They have succeeded in stimulating both interest and support as well as arousing disbelief and resistance. By pitting cognitive theory against S–R positions, ingenious experimentation has emerged on both sides of the issue. The resulting sharpening of theoretical issues may eventually revive the interest and the advancement of knowledge achieved during Hull–Spence and Tolman debates which dominated psychological thinking in the forties and fifties.

Although Maier and Seligman's preceding article warrants additional praise, the assignment at hand is to provide a careful analysis of the substance of their manuscript. The strategy adopted is that of the role of the critic in hope that such a critique will produce a positive effect by clarifying issues of contention and pinpointing weaknesses in need of correction. Because of the variety and importance of issues raised by Maier and Seligman and due to the limitations of space and time, the task at hand will not be easy. The following product should not be viewed as complete or definitive nor undeserving of equal critical rebuttal.

PROBLEMATIC DEFINITIONAL ISSUES

In closing their article, Maier and Seligman acknowledge that it must seem to the reader that a great deal of theoretical confusion surrounds the learned helplessness phenomenon. Part of the blame for such misunderstanding must be shared by proponents of the learned helplessness position. The following section is an attempt to highlight those issues and concerns in need of further clarification. One of the major objectives of the Maier and Seligman article is to advance a learned helplessness theory to explain the learned helplessness effect. The authors raise the issue as to what must an adequate theory of helplessness accomplish (p. 16). Their response to this question is threefold: (a) It must account for the three facets of the effects of uncontrollable aversive events (motivational, cognitive, and emotional), (b) it must be testable, and (c)

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Steven F. Maier and Martin E. P. Seligman will reply in a subsequent issue.

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it must be applicable outside the laboratory in explaining any helplessness effects found in the real world.

Clearly other ingredients are needed before a theory can be considered adequate. One such factor is definitional precision. The essence of this issue has been addressed by Feigl (1953):

> This obvious standard of scientific method requires that the concepts used in the formulation of scientific knowledge-claims be as definitely delimited as possible. On the level of the qualitative-classificatory sciences this amounts to the attempt to reduce all border-zone vagueness to a minimum. On the level of quantitative science the exactitude of the concepts is enormously enhanced through the application of the techniques of measurement. (p. 12)

A large part of the attempt for precision in communication involves the operational analysis of psychological terms. As Skinner (1945) warns, *we must explicate an operational definition for every term unless we are willing to adopt the vague usage of the vernacular.*

Procedural Requirements

The first issue of concern centers on the problem of what is precisely meant by the term *learned helplessness.* How is this construct manipulated procedurally and how is it measured behaviorally? Maier and Seligman attempt to define learned helplessness procedurally via their conceptualization of instrumental training as representing environmental contingencies which occur along a two-dimensional space as shown in their Figure 1 (p. 5). According to these authors, such a representation permits the operational specificity of the concept controllability. From such a conceptualization, a response R stands in a relation of control to a reinforcer RF if and only if $p(\mathrm{RF}/\mathrm{R}) \neq p(\mathrm{RF}/\bar{\mathrm{R}})$. In other words, a response will procedurally change what the subject receives. On the other hand, a response R stands in relation of noncontrol to a reinforcer RF if and only if $p(\mathrm{RF}/\mathrm{R}) = p(\mathrm{RF}/\bar{\mathrm{R}})$. That is to say, a response is independent of reinforcement and will not change procedurally what the subject receives. It is this latter case, in which the conditional probability of an outcome, given a specific response, does not differ from the conditional probability of reinforcement in the absence of that response, that is procedurally required for producing a learned helplessness effect. The learned helplessness effect then appears to be defined as a decrement in acquisition of an instrumental behavior by a given subject following exposure to a procedure in which the reinforcers are programmed independently of the subject's responding. It should be noted that Maier and Seligman are referring to aversive reinforcement, and in the vast majority of studies reviewed the learned helplessness effect was obtained following the administration of electric shock.

If the above paragraph represents an accurate account of what Maier and Seligman have outlined in the early part of their article, they indeed have met the necessary requirements for providing operational specificity both in terms of the required procedural manipulation needed to produce the learned helplessness effect, as well as the means of measurement. The reviewer's confusion results from subsequent statements made by the authors concerning their definitional framework as well as issues involving the *utility* and *precision* of their currently stated definitions as they relate to existing findings and to the theoretical structure they present to account for these findings.

First, the issue of the scientific utility of the existing procedural manipulation will be discussed. To obtain a learned helplessness effect Maier and Seligman maintain it is necessary to insure that the aversive reinforcement is administered independent of the subject's responding. Procedurally this manipulation is precise and necessary but is it sufficient? For an operational definition to have utility there should also be a high correlation between the execution of the procedural manipulation and the resulting desired effect. Maier and Seligman are quite aware that the resulting correlation in question is not necessarily high. They point out that exposure to "inescapable" shock produces a deficit on some escape tasks but not on others (p. 39). For example, an FR-1 shuttling paradigm used with rats does not usually produce the learned helplessness effect and similar results have been found with rats on an FR-2 lever pressing task (Seligman & Beagley, 1975). The authors also acknowledge that in the studies with dogs where the learned helplessness effect was obtained, one third of the subjects seemed completely normal in that, like naive dogs not exposed to inescapable shock, they escaped efficiently. In some of the experiments only half of the subjects showed the desired effect (Maier, 1970).

Although the learned helplessness procedure does not usually produce a learned helplessness effect in rats exposed to an FR-1 schedule, data are presented by Maier and Seligman which suggest the effect can be obtained with an FR-2 shuttle and an FR-3 bar press response. They observe that one critical factor in explaining these differences emerged: "The response used in the test for learned helplessness must be difficult, and not something the rat does very readily" (p. 8). They further reason that crossing the shuttle box once for a rat is a "high probability initial response to shock" and perhaps represents for the rat a species-specific defense response (p. 23). If this is the case, then the procedural definition for producing the learned helplessness effect in rats could be made more precise by operationalizing task difficulty or species-specific defense responses. However, from this reviewer's viewpoint, the species-specific defense response concept (Bolles, 1970) is a very vague notion at present and lacks adequate operational precision within a given species or between species.

To pursue this matter in a fruitful sense, Maier and Seligman must come to grips with such questions as why crossing the shuttle box for a dog, a cat, or a fish is not a species-specific defense response to shock when it surely is an effortless response for these organisms and is quickly learned? Furthermore, why does this label not apply to a human removing his hand from the grid of a shuttle box since such a response readily can be performed (Hiroto, 1974)? Whether the critical parameter involves response effort or perhaps an intrinsic feedback requirement, the data suggest the original procedural definition must include additional rules.

To explain away the problem associated with intraspecies variability, Maier and Seligman conclude: "We believe that the prior history of these dogs before they arrived at the laboratory may determine whether a naive dog will show a learned helplessness effect and whether a dog given inescapable shock will be immune to this effect" (p. 7). If this is the case, then it would seem to follow logically that if subjects were administered the learned helplessness procedure shortly following weaning, the 33⅓% figure reported as representing the percentage of "immune" subjects tested at maturity should be markedly reduced. The only developmental study reported was a recent study conducted by Hannum, Rosellini, and Seligman (in press). Rats who were given inescapable shock following weaning did display the learned helplessness effect at 90 days of age. Unfortunately, the percentage of subjects who were "immune" in the inescapable conditions was not reported in the draft of Hannum et al. However, speculating from the data presented it appears that some of the inescapable subjects emitted the escape response on subsequent testing. Of special interest would be a design where subjects are tested for learned helplessness effect immediately after weaning. If the percent of immune subjects is not markedly attenuated, proponents of the learned helplessness position will be forced either to postulate a genetic position or to argue that immediate testing following weaning is not early enough to account for differences in past histories. If genetic differences are appealed to, the term *learned* will have to be dropped when describing the effect. If differential preweaning histories are appealed to, the resolution of this issue will be difficult to test. Whatever the outcome, if past history is an important variable to account for the variance then the procedural definition should be refined to operationally reflect this variable. In the absence of this refinement, post hoc explanations will flourish and arbitrarily determine what is referred to as the learned helplessness effect.

A similar problem arises when at one point in their article Maier and Seligman appear to alter their original procedural definition. In criticizing the Bracewell and Black (1974) test procedure, the authors state: "In order for an effect to be called a learned helplessness effect, it must be demonstrated that the effect is caused by the *controllability* of the events delivered during pretreatment" (p. 22, italics theirs). Later in the article they also criticize Weiss and his colleagues on similar ground. Yet earlier in the article Maier and Seligman define lack of control procedurally by stating the formula $p(\mathrm{RF}/\mathrm{R}) = p(\mathrm{RF}/\bar{\mathrm{R}})$. The learned helplessness effect is considered to have occurred if this procedural manipulation is made and a decrement in responding is found during the test period. Both aspects of this latter definition were met in the Bracewell and Black experiment and in the Weiss, Glazer, and Pohorecky (1975) series of experiments. How can Maier and Seligman not define the effect as learned helplessness when they use this same definitional scheme to label a host of other studies as producing the learned helplessness effect?

The whole theoretical issue between an S–R position and the cognitive position of Maier and Seligman is one based on the issue of whether the procedural manipulation of learned helplessness does or does not produce differential reinforcement of motor responses. *This is an empirical question.* If Maier and Seligman pursue this issue they are in effect saying that if any experiment employs their procedural definition for learned helplessness and then empirically shows the effect obtained to be a result of differential reinforcement, by definition the obtained effect cannot be referred to as being a learned helplessness effect. If, however, one does not empirically isolate a differential reinforcement effect, the "true" phenomenon of learned helplessness has been obtained. In commenting on the Weiss et al. (1975) experiments, Maier and Seligman state: "The basic similarity is that in both cases [Maier & Seligman experiments and Weiss et al. experiments] animals failed to escape shock, but there may be many roots to producing such a deficit, and they may not reduce to one explanation" (p. 37). Thus, the basic issue remains: What are the procedural requirements needed, stated operationally in terms of both input and/or output variables, before an investigator can be assured, prior to conducting an experiment, that the necessary and sufficient conditions have been specified to produce the learned helplessness effect described by Maier and Seligman?

Measurement Requirements

It should be clear from the above that the procedural requirement of programming the reinforcements independent of subjects' responding is a necessary but not a sufficient manipulation to insure the learned helplessness effect. Perhaps the large portion of unexplained variance obtained by the above manipulation can be lessened by either limiting or redefining the response mechanisms under study and/or isolating the reinforcement parameters required to produce the effect.

It will be recalled that the learned helplessness effect was defined by Maier and Seligman as a decrement in acquisition of an instrumental behavior by a given subject following exposure to a procedure in which aversive reinforcers are programmed independent of the subject's responding. The authors have documented, by referring to a large body of literature, the fact that such a decrement can result following exposure to such a procedure. At points throughout their article the authors appear to enhance precision in defining their effect by isolating certain response topographies which they believe are more sensitive to producing the desired decrement in responding. One example of such an attempt previously mentioned is their use with rat subjects of FR-2 shuttling and FR-3 bar press procedures. Another example involves their attempt at times to limit the learned helplessness effect to measurement of escape responding and at other times to define the effect even more precisely by requiring the definition of the dependent measure used to include a failure to escape.

First, the issue of whether or not Maier and Seligman intended to limit the measurement of the learned helplessness effect to escape responding will be considered. From their statement made at a later point in their article this clearly appears to be the case. In their own words:

> First, the experiments discussed by Anisman and Waller are not directly relevant to the learned helplessness effect. They are all studies of effects on *avoidance,* not escape. . . . The learned helplessness effect is a failure to escape—to flee from shock itself—a more profound debilitation than failure to prevent shock. (p. 22, their italics)

However, earlier in their article, references are made to the term *avoidance* in describing the learned helplessness effect. For example, the authors state:

> Further, after uncontrollable shock, dogs are not only debilitated at fleeing from the shock itself (escape), but they also seem to be unable to prevent or "avoid" shock. . . . The helpless dogs failed to avoid, just as they had failed to escape. (p. 8)

It would appear that the issue of whether or not a decrement in avoidance learning is to be considered a learned helplessness phenomenon is in need of clarification. Further confusion arises regarding the boundary conditions required in limiting response classes when the authors discuss the generality of the learned helplessness effect.

> So we believe that the psychological state produced by uncontrollability may undermine response initiation quite generally. . . . Furthermore, these motivational deficits may not be limited to shock or even noxious events. Escape from frustration, aggressive behavior, and even the propensity to solve anagrams seem to be undermined by inescapable aversive events. Conversely, uncontrollable rewards disrupt escape from loud noise, learning to procure food, and competitiveness. (pp. 12–13)

The second issue of concern is whether Maier and Seligman intended to define the helplessness effect even more precisely by requiring the definition of the dependent measure used to include a failure to escape. At many places in their article this clearly appears to be their intention. For example, acknowledging that rats frequently fail to produce the learned helplessness effect after receiving inescapable shock, the authors state: "Unlike dogs, a rat given prior inescapable shock was typically only a bit slower to escape shock on the first few trials, or slower to acquire avoidance—it did not fail to learn" (p. 8). By isolating the learned helplessness effect to a failure to learn (a more general statement) or to a failure to learn to escape shock (a more limited statement) the authors have clearly isolated the phenomenon under discussion. If this strategy was indeed their intention, the point should be incorporated into a more formal definitional framework. The reason for this latter request is because in a number of other parts of their article the empirical definition of a helpless effect seems to change from one of "failure to learn" to one of demonstrating a decrement in performance depending upon the study under question. For example, in describing the helplessness effect in the Braud, Wepmann, and Russo (1969) study, the authors state that their yoked group (helpless group) was poorest at escaping from water (p. 10). In fact the subject did escape although a decrement in performance was obtained.

In a number of other studies where the helplessness phenomenon was supposed to have been demonstrated, the term *retarded* is used by Maier and Seligman to describe the resulting decrement. A cursory inspection of some of these studies, especially those involving human subjects, indicates a complete failure to learn did not occur. If a learning–no-learning definitional framework is to be maintained, then a number of studies which Maier and Seligman cite to support their position should be eliminated from their article. Furthermore, if such a dichotomized criterion is to be advanced, those studies which used parametric statistics to show the effect should be reanalyzed with nonparametric tests. In either case it would be helpful if future studies would publish the individual subject distribution of responding so the exact nature of the distribution obtained could be scrutinized. It is the author's opinion that if the learned helplessness effect were defined solely as the absence of any escape responding many of the studies reviewed would not have yielded a reliable difference between experimental and control subjects.

Lastly, a comment is in order concerning Maier and Seligman's statement that it does not matter if the inescapable shock is or is not preceded by a signal (p. 7). If a signal is permitted to be introduced into the context of a learned helplessness paradigm, then this reviewer has difficulty discriminating procedurally between a learned helplessness procedure and a classical conditioning fear procedure where the reinforcement is also programmed independent of the subject's responding. As McAllister and McAllister (1971) have demonstrated, the classical fear conditioning procedure is frequently followed by an increment in instrumental responding rather than a decrement. That is to say, when provided an opportunity, the animal learns to leave the apparatus in which inescapable shocks were previously administered.

THE LEARNED HELPLESSNESS TRIADIC DESIGN: A RECONSIDERATION

Maier and Seligman raise an important question when they ask how one can determine whether the learned helplessness effect results from experiencing uncontrollable outcomes as opposed to merely experiencing the outcome itself (p. 6). Their answer relies on the use of a "triadic" procedure which is described by them as representing "a simple and elegant experimental design" (p. 6). The triadic design involves the use of three groups. The first group receives as its pretreatment manipulation an experimentally manipulated contingency that can be directly altered by a given response. The second group receives as its pretreatment exactly the same physical outcome as its counterpart in the first group via a "yoking" procedure with the stipulation that yoked subjects' responses have no direct effect on procedurally modifying these outcomes. The final group receives no pretreatment manipulation. At a later point in time all groups are tested on a new task where the procedural contingency used can be altered directly by a given response.

The triadic design is viewed by Maier and Seligman as a direct test of the hypothesis that the cause of helplessness is learning that shock is uncontrollable, and not shock per se, causes helplessness. In other words it is used as a control for stimulation per se. The yoked group, then, is the experimental group under investigation in helplessness experiments. It should be noted, however, that the yoked procedure was originally introduced into the field as a control group. The design was developed in an attempt to determine whether the temporal relationship between an event and response is related to the observed effect obtained. The yoked group in this case serves as a control to determine whether the above contingency is indeed the important factor. Despite differences in usage and semantic labeling, the yoked procedure in both cases is identical, as is the resulting behavioral effect. Within the triadic design, the decision of Maier and Seligman to select the yoked condition as the main subject of study has led to an important contribution. As their review attests, the behavioral outcome of the yoked subjects has been demonstrated to lead to a number of behavioral outcomes, including nonescape, loss of weight, and other effects interpreted as signs of stress.

Maier and Seligman clearly document the importance of viewing the yoked procedure as an experimental condition. However, in an earlier, well-conceptualized paper, Seligman (1969) has questioned the validity of using such procedures as the yoked condition for a control group. In Seligman's own words:

> When one runs a control group, one must be alert to the possibility that one operational difference may produce several effects. . . . If multiple effects are produced by the control manipulation, the investigation must determine how these effects interact with the main dependent variables before making any interesting causal inferences. Thus, the present author has suggested that one cannot determine a priori on operational grounds alone what the appropriate control group should be. One controls not only for operations, but also for the results those operations are expected to produce. (p. 490)

If Seligman is correct that the *presence* of additional factors or variables produced as a result of a single control operation potentially confounds interpretation of the absence of instrumental responses for which the control is intended, then would it not be reasonable to expect the counterpart to be equally plausible? That is to say, is it not logical to consider the possibility that the *absence* of these *same* additional factors as a result of the opposite control operation must necessarily confound interpretation of the presence of instrumental responses being controlled for in the triadic design?

A closely related criticism of the triadic design may be found in the now classic paper on the systematic effects of random error within the yoked control design outlined by Church (1964). It is interesting to note that Maier and Seligman's article under discussion does not mention Church's position while discussing the merits of the triadic design. In fact, on cursory examination of the helplessness literature, only one reference to Church's position was found (Seligman, 1975). In a footnote, Seligman takes the position that Church (1964)

> has argued against the use of yoked group as a control group for instrumental learning. This argument is not relevant to experiments on helplessness in which the yoked group is the experimental group, and the other groups are control groups. (Seligman, 1975, p. 190–191)

While it is true that Church speaks specifically to the issue of the yoked group as a control group for instrumental conditioning, it is not clear that this precludes its application to the triadic design. As a control group, the yoking procedure involves a number of subjects that are paired on some basis. One member serves as the experimental subject, the other as the yoked control. The experimental subject is placed in an instrumental learning situation, in which an event occurs if it makes a particular response; the yoked control subject is put into a situation in which an equivalent event occurs if its yoked experimental subject makes the response. As Church (1964) pointed out, the critical characteristic of this design is that an experimental subject determines the presentation of events to its control subject so that the control subjects and experimental subjects receive the same number and temporal distribution of events.

Following Church's arguments through, one must consider a situation in which the null hypothesis is true, namely, that the temporal relationship between the response and the event is irrelevant, though one in which the event does have an effect on behavior. In the yoked control design, individual differences in subject variables do not necessarily result in random errors, but rather can produce a constant error systematically biasing one group against another. One important and reasonable common individual difference in conditioning procedures will be the effectiveness of an event. If an experimental subject for which the event is effective is matched with a yoked control subject for which the event is equally effective, no reliable difference in the percentage of conditioned responses will occur. A similar result will be obtained in those cases for which the event is equally ineffective for both subjects. However, if an experimental subject that is affected by the event is matched with a yoked control subject that is not, the experimental subject will make substantially more responses than its matched control. On the other hand, if an experimental subject not affected by an event is matched with a yoked control subject that is affected, neither will make many responses. The experimental subject will rarely make a response, by assumption, and the control subject will not make many responses because it only occasionally receives an event. An algebraic summation of these four potential outcomes will yield a systematic bias in favor of the experimental group as performing a greater number of instrumental responses despite the irrelevance of the response event contingency. Church is addressing himself to a potential behavioral outcome which is the result of an interaction between a subject variable and a procedural manipulation. The hypothesis generated is that the effect of an event on a response is related to the temporal relationship between the event and that response. Given the systematic biases outlined above, the experimenter would falsely reject the null hypothesis with a higher probability than the level he had set for the Type 1 error.

In Maier and Seligman's discussion of the triadic design the labels for experimental and control groups are reversed, although the procedural manipulations are identical and so, presumably, the behavioral outcomes are equally identical. The hypothesis generated is that the behavioral outcome (decrement in instrumental escape behavior) is related to prior exposure to the absence of a temporal relationship between the event and response. If the same systematic bias results as stated above, the experimenter will again falsely reject the null hypothesis, thereby assuming a relationship when in fact there is none.

The application of Church's (1964) position to the learned helplessness position is potentially serious. One of the most powerful rebuttals to the competing-motor response theory of learned helplessness put forth by Maier and Seligman is the Maier (1970) study in which dogs given passive avoidance training were compared with dogs given inescapable shock. Presumably, if inescapable shock produces motor responses such as freezing or immobility, incompatible with later shuttle box performance, and immobility or passive avoidance is trained in a control group, there should be no difference in later shuttle box escape/avoidance performance. On the other hand, if escape/avoidance decrement is related solely to prior exposure to uncontrollable shock presentations, then the control group (controllable shock by passive avoidance) should be superior to the yoked experimental group in later shuttle box performance. As Church points out, presumably there will be some variation from moment to moment in the severity of shock, as a result of variation in density and locus of the shock, degree of grid contact, etc. Whenever the control subject receives a particularly painful shock, it may be able to escape quickly, but its matched experimental subject is not as fortunate. Perhaps the result of such moment-to-moment variation in shock plays an important role here as opposed to simply the lack of control of shock. Recent evidence by Anderson and his colleagues (to be discussed later) would seem to indicate that the issue of shock parameters, regardless of the nature of shock control, is of extreme importance in determining the learned helplessness effect. Clearly, more parametric work is needed in the alternatives to the yoked procedure such as those outlined by Church and examined by Anderson (Note 1) before the issue is resolved. Until such time, however, experimenters who uncritically adopt the use of the triadic design must contend with the potential for systematic error and faulty inferences.

LEARNED HELPLESSNESS THEORY

One of the major objectives of the Maier and Seligman article is to advance an adequate theory to account for the empirical findings generated by the learned helplessness paradigm. The theory advanced is described as involving three separate stages. The first ingredient consists of the organism receiving information that the probability of the outcome is independent of performing a given response class. This stage is achieved via a yoked procedure in the context of the triadic design employed in learned helplessness research. The second and critical stage involves the organism registering and processing cognitively the information about the contingency outlined in the first stage. This transformation can be subdivided into two processes: (a) learning that a contingency exists concerning the independence of responding and outcome, and (b) developing the expectation that responding and outcome will remain independent on future trials. Concomitant with the second stage is a reduction in the motivation to control the outcome. The final stage involves the generalization or transferring of the expectations developed that responding and outcome are independent to new learning situations. The outcome of this generalization is behaviorally referred to as the learned helplessness effect.

Maier and Seligman considered the theory presented to be straightforward (p. 16). Indeed the deductive steps taken to outline their position can be viewed as representing a logical sequence of steps bridging the gap between their procedural manipulation and their dependent measurement of the learned helplessness finding. Despite the internal consistency offered regarding the interrelationship of their postulated theoretical constructs, a number of lingering questions immediately present themselves. How does one determine, independent of measuring the outcome variable, when or whether a given procedural manipulation has been registered cognitively? Once registered, what are the rules and measurement criteria for determining whether or not an "expectation" will or has developed, and whether or not it will generalize to future trials? What are the parameters responsible for facilitating this generalization process, and what are the rules which determine its boundary conditions? What exactly are expectations, how are they identified, and more precisely how are the critical intervening variables anchored or operationally linked to antecedent conditions?

To the theoretical purist, such questions are of paramount concern and not new to the position being discussed. One needs only to recall the voluminously critical writings generated by philosophers of science concerning the cognitive theory of Tolman (1932). For example, Osgood (1953) summarized the viewpoint of many of the authors of the time.

> Tolman has shown a magnificent lack of concern over the details of behaving. Having cognitions and demands; appropriate behaviors just appear spontaneously. As White (1943) puts it, 'Since their behavior is not necessarily a "response" to a "stimulus" it is called simply behavior. . .' (p. 171). This lack of concern has certain advantages, chiefly that the important phenomenon of response equivalence can be accepted without explanation. It also has a serious disadvantage from the standpoint of theory, namely, that there is a gap in the inferential sequence which makes detailed predictions impossible. . . . Because of this gap in the predictive sequence, one gets the impression (while reading Tolman's work) of a disembodied rat floating along the cognitive lines of its expectations, busily plotting maps of the experimental field—all quite isolated from the corporeal, behaving muscles which move it here and there. (p. 391)

Perhaps it is inappropriate to resurrect the issues of antiquity and suggest they have relevance to modern-day cognitive theory. But if criticism is valid, it should have no time restraints imposed upon it. Let us consider those points raised by Osgood which may be appropriate to Maier and Seligman's formulation.

In addressing themselves to the issue of what an adequate theory of helplessness must achieve, Maier and Seligman state that such a position must account for the three facets of the effects of uncontrollable aversive events: motivational, cognitive, and emotional. Indeed, the model offered includes three such constructs. Perhaps in reviewing the rationale upon which the three ingredients of the model obtained their empirical base, an independent definitional framework will emerge for each construct. If such is the case, Osgood's criticism of Tolman, that a magnificent lack of concern exists over the details of behaving, will indeed be deemed inappropriate for the purpose of our present discussion.

Maier and Seligman maintain that deficits in motivational, cognitive, and emotional components often ensue when an organism experiences uncontrollable events. Let us analyze the definitional properties of each of these components separately.

Motivational Component

The authors state that once exposed to the learned helplessness procedure the motivation to respond in the face of later aversive events seems to wane. They suggest that the deficits produced in the component "are in many ways the most striking" (p. 7). Furthermore, a large section of their article (pp. 7–13) is devoted to documenting their contention that such a motivational deficit exists.

Unfortunately, after a careful review, it appears to this writer that a motivational deficit is defined by Maier and Seligman excluding references made to anecdotal observations, as a decrement in performance or a failure to learn. But the latter measure also defines the learned helplessness effect. Are Maier and Seligman seriously suggesting that the outcome measure or performance variable is equated with the concept of motivation without independently attempting to separate motivational effects from reinforcement effects? From this writer's analysis the outcome of not performing a given response class in no way a priori suggests anything definitive about the energy or motivational properties of the organism. It seems equally conceivable from data obtained from the passive avoidance literature that an organism can be under a high-drive state and at the same time behaviorally be immobile depending upon the interaction of habit and drive. Clearly, an independent set of input and output variables must be established before the motivational properties of the organism can be assessed. As Osgood noted (1953, p. 392), linking theoretical constructs only to the subsequent dependent behavior eventuates in the development of a tautology.

Cognitive Component

Much of what was said concerning the definition of motivational properties can also be applied to determining whether a cognitive deficit exists. The measurement of a cognitive deficit also appears to involve the performance variable used to define the learned helplessness effect. A cognitive deficit is reported to occur following the learning that reinforcement and responding are independent, when the organism's ability to learn that two sets of stimuli depend upon one another is demonstrated to be retarded. Although the evidence presented by Maier and Seligman, which mainly comes from human studies, is substantial, the existence of effects noted in fish, rats, cats, dogs, and people, and Seligman (1975, p. 28) even these alternative explanations will be mentioned at a later point when the human experiments are discussed.

The cognitive model of Maier and Seligman apparently has been applied with equal voracity to explaining learned helplessness effects noted in fish, rats, cats, dogs, and people, and Seligman (1975, p. 28) even talks about the helplessness phenomenon occurring in cockroaches. Since the effect appears to have considerable species generality it would not be surprising to learn that helplessness effects are eventually observed in the behavior of *Paramecium* aurelia. If Maier and Seligman are viewing their cognitive model as involving hypothetical constructs in the definitional sense suggested by MacCorquodale and Meehl (1948), one can only be amazed that the brain capacity of a given species is not viewed as an important variable. It would appear that such a factor may well be instrumental in determining whether a helplessness effect will occur, the speed of its occurrence, and the degree by which it can be retained and generalized. Without such a stated parameter we are left with the conclusion that both qualitatively and quantitatively the "expectancy" mechanism of a fish or cockroach is equivalent to that of a man.

Emotional Component

Maier and Seligman observed that the helplessness effect in dogs became dissipated in time after a single session of inescapable shock, suggesting a transient emotional disturbance (p. 14). The authors also appeal to data generated from the experimental neurosis literature. Although a clear distinction between motivational and emotional components eludes this writer, Maier and Seligman suggest that the emotional component can be defined independently of the learned helplessness effect. In achieving this objective the authors have relied upon traditional measure of stress such as the development of ulcers, loss of weight, increased defecation, and decreased drinking behavior (p. 15). Such a definitional scheme is clearly an improvement over what is offered in defining the motivational and cognitive components.

However, some confusion about the importance of this emotional component appears to occur later in the article when Maier and Seligman criticize the position taken by Weiss. In their words

> "Weiss et al. (1975) argue that the learned helplessness effects which we have found are caused by norepinephrine depletion produced by the inescapable shock which *we* deliver. Therefore it is *required* that Weiss and his colleagues demonstrate that *our* inescapable shock conditions produce intense stress and norepinephrine depletion. (p. 31, their italics)

Earlier they mentioned (p. 29) that Weiss defined stress as being measured by gastric lesions, loss of body weight, plasma steroid levels, and fearfulness. Yet Maier and Seligman appear to deny the existence of stress or emotionality variables when referring to their own work. Clearly a greater precision is needed in stating exactly what role their emotional components play, when they occur, what are their time courses, and what are the other boundary conditions needed to determine their role. The Seligman and Johnston (1973) paper does represent one such effort in this direction.

In summary, if Maier and Seligman are intent upon offering a theoretical analysis to explain a behavioral outcome referred to as the learned helplessness effect, a clear definition of each process in the chain is required along with specified methods for measuring the process. Since the evidence provided by Maier and Seligman suggests the need for the postulation of covert mediational processes, which was the strategy followed, the resulting constructs must be linked to antecedent conditions if predictive power is to be enhanced. Defining constructs only in terms of subsequent, dependent behavior leads to circular reasoning. Clearly some of the criticism offered must be tempered in fairness to Maier and Seligman since their theory is clearly in its developing stages. Because of the cognitive nature of the theory, time must be granted for the establishment of adequate measurement instruments to elevate their constructs to the domain of direct manipulation and observation. It should also be noted that Maier and Seligman constructed their theory partly by the process of exclusion. In their own words:

> We have found it difficult to even approach the sort of phenomena that we have tried to explain within an S–R framework, and have found the cognitive theorizing to be more fruitful and to reflect more accurately those processes that we feel to be reflected in behavior. (p. 57)

Let us now turn our attention to some of the critical phenomena which guided them to their position.

Alternative S–R Positions

In the final section of their article, Maier and Seligman review alternative theories to the learned helplessness hypothesis. The alternative positions were divided into motivational and motor accounts. They are primarily S–R in conception and designed to account for data obtained at the infrahuman level. Considerably more attention is given to the motor accounts which are subdivided into incompatible motor response theories and the motor activation deficit hypothesis. It is in this final section of their article that the full power of the helplessness hypothesis emerges. The predictive implications of the helplessness model are developed in an impressive manner. With the skill of master swordsmen, Maier and Seligman dissect the motivational and incompatible motor response theories with such convincing data and equally devastating arguments that one wonders whether or not their blows are fatal.

The motor activation hypothesis fairs somewhat better in Maier and Seligman's attack. The authors even acknowledge (p. 36) that Weiss et al. (1975) have demonstrated a number of very interesting effects and that the interpretation of their own data is likely quite correct. However, Maier and Seligman deny that the data obtained by Weiss et al. can be labeled learned helplessness effects. The latter conclusion is based, in part, upon the procedural differences employed in the Weiss et al. series of experiments. Maier and Seligman's arguments appear at times to be presented cogently but at other times seem to be immersed in definitional issues as noted earlier in this article. Clarification of the issues advanced must await further experimentation.

Clearly, Maier and Seligman have amassed a strong, critical, data-based argument against incompatible motor response positions. At present the bulk of the data appears to support their cognitive theory. However, a paper presented at the meeting of the Psychonomic Society in 1974 by Anderson, Lupo, Cunningham, and Madden (Note 2) provides the strongest data to date for an interference motor interpretation. These investigators reported evidence obtained from rats which strongly suggests that learned helplessness effects are dependent directly upon the type of shock stimuli used. Inescapable programmed pulsating shock was found to produce no shuttle interference whereas inescapable continuous shock resulted in substantial impairment of shuttle escape behavior. Shock duration effects were also found to be an important variable.

Maier and Seligman will have to address themselves to the issues raised by Anderson and his colleagues. Unfortunately, their data have yet to appear in print, and this writer's urge to highlight their work in detail had to be suppressed out of fairness to Maier and Seligman. To provide a careful evaluation, details of the procedures used must be scrutinized by all concerned. Nevertheless, the data outlined by Maier and Seligman are still in need of explanation if an S–R interpretation is to provide a viable alternative. Most existing incompatible response theories appear to be presented in a post hoc fashion rather than in a more formal integrative way. The latter strategy may well provide the necessary interpretative power to explain at least some of the reviewed data.

Stimulated by Maier and Seligman's statement (p. 41) that their proposed cognitive theory developed, in part, because an S–R analysis was unable to account for the data, this writer undertook the challenge to develop such an interpretive framework. Elements of the model to be proposed are taken from other existing positions. Because of space limitations, the proposed theory will be presented in a less than formal manner. Operational specificity of input and output variables, as well as some of the interacting mechanisms, will not be outlined in detail. However, they should be readily apparent to S–R theorists. At best the model will stimulate additional research. At worst it will provide an easy target for cognitive critics.

TWO-PROCESS REINFORCEMENT THEORY OF ESCAPE LEARNING

Shock onset is believed to elicit at least two unconditioned response topographies. The first involves movement of the area being stimulated away from the point of stimulation and is considered reflexive in nature. The second involves an emotional component and is manifested in unrestrained animals by a general increase in gross bodily movement frequently accompanied by acts of urination and defecation. Both reactions involve movement, the topographies of which are generally heterogeneous and varied depending upon the restrictions of the experimental setting and shock parameters utilized. Shock offset is hypothesized to produce two related, but conceptually different, processes. The first process involves a reduction in painful stimuli and is believed to occur at maximum reinforcement value almost immediately following shock cessation. The second reinforcement process involves a reduction in the emotional state (fear) resulting from the experience of pain.[1] The second process of fear dissipation is considered to be gradual, with the time course of the gradient being dependent, in part, upon the intensity of the shock used. The stronger the shock, the greater the delay needed to reach the point of maximum reinforcement value. Both processes will tend to strengthen directly those response classes which immediately precede the point of maximum reinforcement value. Pain reduction, however, is viewed as producing a stronger per trial reinforcement effect.

First let us consider the situation in which the animal is exposed to a contingency in which a given response class will modify the shock duration (escape procedure). As stated before, the initial presentation of shock onset will markedly increase a subject's activity. If the escape response required involves a response class *similar* to that elicited by the unconditioned effect of shock, the probability of the escape response occurring will be high. The first and subsequent occurrence of the escape response will be reinforced directly by pain reduction, leading to the learning (shorter escape latencies) of the response class under consideration. It should be noted that the response class learned is homogeneous in nature, with all competing response classes being inhibited as learning increases. If the escape response under consideration is *not similar* to or incompatible with the elicited unconditioned responses, the probability of the first occurrence of the desired response will be low. With repeated nonescape trials and with longer shock duration periods, the probability of escaping will continue to decrease. The latter will occur because (a) none of the various response classes emitted will be selectively reinforced and (b) continual shock trials may well result in punishment effects similar to those suggested by Bracewell and Black (1974) and by Anderson and his colleagues (Note 2). Thus, the probability of the occurrence of escape learning will be a direct function of the similarity of the response class selected for testing and the response classes elicited by unconditioned effects associated with shock parameters.

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[1] The author would like to acknowledge the influence of Judson S. Brown for first bringing to my attention the potential effects of postshock emotionality, which grew out of a lengthy interchange a number of years ago concerning a different area.

The second reinforcement process, a reduction in fear, will strengthen whatever response class is present while the process is taking place. If the response class occurring on a trial-to-trial basis is variable, the accrued reinforcement effects to a given response topography will be small, producing little systematic effect on the response class reinforced by pain reduction. However, if the response class is initially relatively stable from trial to trial, its strength will systematically increase with trials. Furthermore, this habit strength is expected to increase progressively with repeated trials at respective points along the fear gradient at least up to the point of shock offset. It is therefore conceivable that with established escape learning, shock onset may produce one set of response classes and shock offset a different incompatible set. This latter possibility is supported by the observation that for a number of animals (especially rats) activity level following shock offset appears to markedly decrease. Empirical evidence of intertrial movement activity following avoidance training also supports this notion (Shipley, Mock, & Levis, 1971). Thus, if the escape response involves movement, this response will be reinforced by pain reduction. If immobility initially occurs following shock offset, this response class will be reinforced by fear reduction.

Let us now apply this two-process reinforcement model to an inescapable shock procedure. The reinforcement effects of pain reduction still occur following each shock offset period. But the probability of a given response class being strengthened systematically early in training is low, given that shock duration is programmed independently of the response elicited. If relatively few trials are given, this procedure should have little effect on subsequent escape training since the reinforcement effects are distributed among the various response topographies elicited by shock onset. However, with a large number of such trials, the possibility that immobility (freezing) will become the dominant response class is expected to increase for the following reasons: (a) Since onset of shock is expected to elicit response topographies involving movement, the continual presentation of shock following the onset period may, over trials, result in a systematic punishment of movement activity. Such an effect would increase the probability that inactivity would become the dominant response pattern. This possibility becomes more likely with the use of a yoked procedure since duration effects will vary from trial to trial. (b) As Anderson and his colleagues (Note 2) suggest, certain types of shock parameters may increase the probability of the freezing response occurring during shock presentation. And (c), if a strong habit of immobility develops immediately following shock offset as previously suggested, and if a dominant response topography is not present during shock presentation, the stronger reinforced habit of immobility may transfer back to the shock presentation period.

Once the frequency of inactivity increases during shock presentation, immobility, as was the case with the escape response, will come under the reinforcement control of shock offset (pain reduction). If the two reinforcement processes (pain and fear reduction) produce similar response topographies, the reinforcement effects should summate. When the inescapable shock subjects are then transferred to an escape paradigm, those subjects that previously have been reinforced for freezing will have a greater tendency than escape controls to remain immobile in the presence of shock. Thus, the probability of not learning the escape response will be high, producing the so-called learned helplessness effect.

One obvious implication of the above model is that the mechanism outlined for producing the learned helplessness effect would not be expected to occur in every animal. For the model to predict learned helplessness, immobility must be demonstrated to have become the dominant response pattern following shock onset during exposure to inescapable trials. The factors responsible for producing immobility during this period are not under experimental control, increasing the chance for individual variation. Furthermore, it is conceivable that for some subjects fear reduction may systematically strengthen activity rather than inactivity, leading to the possibility of a subtractive rather than additive effect of the two reinforcement processes. Since degrees of activity can be defined empirically, one need not appeal to unknown events involving prior history to account for the established variability of the learned helplessness phenomenon. By measuring activity directly during the inescapable shock period, it should be possible to predict for each subject whether or not a helplessness effect will occur.

A number of other predictions can be made from the proposed two-process theory, but for purposes of this article, the discussion will be confined primarily to those "critical" studies stated by Maier and Seligman to be unanswerable from an S–R position. They fall into five general areas.

1. The first issue considered is why rats failed to manifest the learned helplessness effect in the context of FR-1 shuttling. According to the proposed models one need not appeal to a species-specific defensive reaction interpretation. Following exposure to the triadic design, inescapable subjects are expected, upon the introduction of the escape contingency, to possess a stronger tendency toward immobility upon reexposure to shock than the escape and no-treatment subjects. With moderate or high levels of shock, some activity upon the initial presentation of shock should occur. As noted earlier if the activity elicited by shock is similar in response topography to that of the required escape response, the chances will increase that the desired response will occur and be reinforced. In the FR-1 studies with rats the response required usually involves the rats crossing from one compartment to another. The above response topographies are considered relatively similar to the topographies elicited by unconditioned effects of shock. Although inescapable shock subjects are expected to perform some activity response during escape training, the frequency and range of such activity should be reduced when compared to escape and nontreated controls. Although learning may well occur for the inescapable subject, an initial decrement in responding is predicted. This expectation is confirmed by the finding reported in the literature (Maier & Seligman, 1976).

The usual FR-1 shuttle escape response required in the dog studies is defined as a leap over a shoulder-high barrier, a response topography considered to be more removed from that elicited by unconditioned effects. Since the range and frequency of activity responses of inescapable subjects is considered to be reduced, the probability of the desired response class occurring is considerably lower than the same probability generated for the response class defined as escape behavior in rat studies. The same effect obtained in dogs, with everything else held constant, should be obtained in rats if the escape behavior involves a leap over a high barrier. This prediction receives some support from the Weiss et al. (1975) study. Detrimental effects were increased for rats in an inescapable stress group when the height of the barrier in the shuttle box was increased, and decreased when the escape response was defined as poking their nose through a hole.

According to the two-factor position, FR-1 shuttle box responding in rats should also produce the learned helplesness effect if number of inescapable trials are significantly increased and/or if higher shock intensities are employed. Weiss et al. (1975) again provide data which tend to support both of these predictions. In fact, these authors are criticized by Maier and Seligman (p. 35) for making these recommended manipulations. It should be noted, however, that the Weiss et al. inescapable condition did not always involve exposure to electric shock, so direct confirmation of the above predictions is still in need of testing.

In the FR-1 shuttle box studies where escape or avoidance was obtained, the two-process theory would predict that FR-1 shuttling should *extinguish* more rapidly in groups given previous exposure to inescapable shock than in control groups. The prediction is based on the assumption that, because of previous reinforcement effect, a stronger habit strength for immobility still exists for the inescapable subjects when compared to controls. Once the reinforcement for substaining the escape behavior is removed or reduced, the probability is higher for the inescapable shock subjects that the response class of nonresponding will be disinhibited. This prediction is consistent with the finding reported by Testa, Juraska, and Maier (1974).

(2) Maier and Seligman (pp. 24–25) discuss two cleverly designed experiments conducted by Maier and Testa (1975) which effectively argue against incompatible motor response theories. In the first study Maier and Testa found that while inescapably shocked rats failed to learn ordinary FR-2 shuttling, they learned as well as nonshocked controls when there was a 1-sec interruption of shock after the first crossing of the FR-2 procedure. In their second study Maier and Testa found that a delay in shock termination as short as 1 sec severely retarded the acquisition of FR-1 shuttle box escape responding in rats previously exposed to inescapable shocks, but had no effect on subjects not previously exposed to inescapable shocks. Maier and Seligman conclude after reviewing these studies:

> The incompatible motor response hypotheses cannot handle these data. It is difficult to conceive of a motor response that would interfere with FR-2 shuttling when there is no shock interruption, but not when there is. Similarly, what motor response could be incompatible with FR-1 shuttling if there is a short delay in shock termination but not incompatible when there is no delay? (p. 25)

The two-process model can explain these data in the following manner. In Maier and Testa's first experiment, rats learned the FR-2 shuttling if a 1-sec interruption occurred on each trial after the first crossing of the imposed FR-2 escape contingency. The issue of why rats failed to learn on ordinary FR-2 shuttling procedures needs to be discussed first. Three possible explanations can be advanced. First, the escape response topography required by the FR-2 procedure is more unlike the topography of responding elicited by the unconditioned effects of shock. Although this statement is true for both inescapable and corresponding controls, as previously discussed, inescapable subjects are expected to generate initially a more limited range and frequency of activity response when compared with the controls. Furthermore, the inescapable subjects have a previously reinforced history of remaining immobile in the presence of shock and, if a difficult response topography represents the requirement for learning, the probability of activity being elicited decreases with each trial on which activity is not directly reinforced. Second, FR-2 schedules result in a greater per trial shock duration period when compared with an FR-1 schedule. Under an FR-2 schedule, the time span from onset of escaping and termination of the response is at least twice as long as required by an FR-1 schedule. Thus, the increased duration required for the response results in a delay in reinforcement effect (pain reduction). This delay weakens the transfer of habit strength to the start phase of the escape response chain. For inescapable subjects, the lack of a strengthening effect at the start of the escape chain will increase the likelihood of the immobility habit being reinstated to the shock stimuli. And third, longer shock durations could conceivably result in a punishment effect of escape responding, especially if the nature of the escape response class or shock parameters result in multiple onsets of shock.

Returning to the discussion of Experiment 1 of Maier and Testa (1975), the 1-sec shock delay period in effect provides for the inescapable subjects more immediate reinforcement of pain reduction like that which occurs in the FR-1 schedule. Two responses are involved instead of one. Shock offset at this point also can serve as a signal for decreased activity as discussed earlier for inescapable subjects. This latter effect is expected to exist especially when rats are used as subjects. The process of becoming inactive is quickly punished by the return of shock onset which in turn increases the probability that shock onset will elicit escape activity. Again, the second escape response is quickly followed by another reduction in pain which in turn provides another strengthening effect of escape responding.

The interpretation of Maier and Testa's second experiment should be apparent from the analysis of their first experiment. It will be recalled they reported that a delay in shock termination as short as 1 sec severely retarded the acquisition of FR-1 shuttle box escape in rats previously exposed to inescapable shock. Actually they manipulated three time delays, 0, 1, and 3 sec. *Thirty* FR-1 escape/avoidance trials with 0 delay preceded the introduction of the three delay periods. A control of inescapable subjects was also included in the design for the effects of extra shocks received during the delay procedures. For the control subjects, a barrier was introduced to block responding. Ten inescapable shock trials were then administered which essentially controlled for the average per trial duration effect obtained for the 3-sec delay inescapable group. The tenth trial was followed by a 0-sec delay FR-1 schedule.

First, the introduction of a 1 or 3 sec shock-termination delay requires the learning of a new, although similar, response topography created by the increased shock duration. Second, pain reduction reinforcement effects are delayed (and possibly punished) because of the increase in shock duration. Both factors should result in weakening the response strength accrued to those response patterns required to initiate the escape response. The weakening effect differentially increases the probability for inescapable subjects to revert back to the immobility response. The effect should be strongest for the 3-sec delay condition. The shock duration control group is not relevant to this analysis since subject did not have the opportunity to continue escaping during the control period. The results were as expected. The 3-sec group differed reliably from all conditions. Inescapable groups 0, 1, and control did not differ from each other but responded more slowly than the groups not previously exposed to inescapable shock.

3. The next set of studies to be discussed involves the basic finding reported by Seligman and Maier (1967) that prior exposure to controllable shock immunizes the organism against the deleterious effects of exposure to uncontrollable aversive events. The design consisted of three phases: (*a*) shock-escape training, (*b*) inescapable shock exposure and (*c*) shock-escape training. According to the two-process model, the above design is more likely to produce escape learning in Phase *c* as a function of response class similarity between Phases *a* and *c*. In the original learned helplessness design, Phase *a* is not presented and the effects of Phase *b* tend to produce a response class incompatible with Phase *c*. The transfer effects from *b* inhibit learning in *c*. But when Phase *a* is added, two effects are possible. First, Phase *a* may transfer its effect to *b*, decreasing the probability that incompatible response topographies will develop in *b*. Second, if incompatible response habits are independently learned in Phases *a* and *b*, the combined effects of both habits (a – b) should transfer to *c* lessening the likelihood of a decrement in learning *c*.

Aware of the above possible interpretation, Maier and Seligman report (pp. 25–26) an unpublished study by Maier and Rhoades. These investigators attempted to produce *different* response topographies in Phases *a* and *c*. The escape response in Phase *a* involved wheel turning while the escape response in Phase *c* involved FR-2 shuttle responding. According to Maier and Seligman:

> It is difficult to argue that learning to turn a small wheel with the paws in a wheel-turn box directly strengthens running in a shuttle box. This wheel-turn escape training should not have an immunizing effect, but it does. (p. 26)

Agreement exists with Maier and Seligman's analysis that the response effects of Phases *a* and *c* in the Seligman and Maier (1967) study are more similar to each other than the response effects of *a* and *c* in the Maier and Rhoades study. A stronger immunization effect should occur in the study of Seligman and Maier (1967). However, Phase *a* in both studies involves gross bodily motor activity. The subtractive or interactive effects (activity or inactivity) of Phases *a* and *b* in the Maier and Rhoades study should still produce an effect on *c*. The resulting effect should be an increase in the frequency and range activity produced in *c* by subjects exposed to Phases *a* and *b* when compared to subjects exposed to Phase *b* in the absence of *a*. By increasing the activity level of subjects receiving an inescapable pretreatment, the probability of escaping also increases.

4. The next section deals with studies that directly attempt to develop escape patterns that are incompatible with the escape response to be learned. Maier and Seligman discuss (pp. 21–22) one such study conducted by Bracewell and Black (1974). These investigators explicitly punished movement by arranging a positive correlation between movement and shock intensity. The procedure produced a retardation in subsequent shuttle box acquisition. Maier and Seligman perceptively criticize the Bracewell and Black (1974) study by noting that the explicit punishment in the Bracewell and Black experiment led to a much smaller escape acquisition decrement than did inescapable shock delivered to restrained subjects. The two-process theory can account for these findings but admittedly the explanation offered is post hoc. Direct punishment by the experimenter of movement responses should result in a reliably faster suppression of activity responses when compared to subjects exposed to a yoked inescapable shock condition. Once immobility is under the control of pain-reduction reinforcement effects. The likelihood of additional activity responses occurring is progressively lower. Because of the lack of a systematic manipulated contingency effect, the immobility response for subjects exposed to the yoked inescapable shock condition is not expected to come under reinforcement control until after a relatively large number of trials. Thus, yoked inescapable shock subjects should initially produce a greater variety of response topographies involving movement. If, as Bracewell and Black suggested, the developing immobility response does result in an inhibition of activity movement, in the learned helplessness group, the inhibition process will effect a greater variety of movement topographies, which should in turn result in a stronger learned helplessness effect.

Maier (1970) applied a different strategy for conditioning incompatible response tendencies. One group of dogs was trained to escape shock by inhibiting head movements elicited by shock. A second group received yoked inescapable shocks and a third group received no pretreatment. As Maier and Seligman point out (pp. 26–27), if inescapable shock produces a learned helplessness effect because it induces freezing or leads to the punishment of movement, then the group taught to inhibit movement as a means of escaping shock should subsequently escape at least as poorly as the group given inescapable shock. The results indicated that the incompatible response group was indeed slow to learn shuttling but all dogs in this group eventually learned. In contrast, half of the subjects in the inescapable group failed to learn the escape response. The notreatment group learned the response readily.

The two-process theory is not concerned with the inhibiting of head movements when the shock was applied to the paws of the dogs in both phases of the study. It is possible that by training dogs to inhibit head movements, gross bodily movement will be markedly reduced when compared with that of inescapable subjects. Interestingly, during this phase, shock electrodes were firmly attached to pads of the subject's hind paws. Shock onset might well have elicited a well-defined "recoil" effect of the hind paws similar to the movement responses initially involved in jumping behavior. If this were the case, the response class under consideration would become strengthened by pain reduction. Because dogs in this group contolled shock duration effect by escaping, one would expect this response class to be defined better than that obtained in the inescapable groups. The latter occurs because shock duration was not under response control allowing for a greater variety of response classes to develop and subsequently be punished. Since the shuttle box escape response was defined as jumping over a shoulder-high barrier, the hind-paw movements conditioned earlier may have produced a greater facilitatory effect for the response-contingent group when compared with the inescapable groups. Since the movement activity was not directly measured, the above hypothesis will have to be subjected to further experimentation.

The final study to be considered under this section involved administration of curare during exposure to inescapable shock. Overmier and Seligman (1967) reported that, after following such a procedure, animals still produced the learned helplessness effect. Maier and Seligman argue (pp. 27–28) that this experiment is a direct disconfirmation of the Bracewell and Black motor hypothesis. For these data to effect the two-process theory, Overmier and Seligman would have had to show that no electromyogram (EMG) responses occurred to the area being shocked. As Black (1971) noted, EMG responses can occur during curarization depending upon the method of administration and other related factors.

5. Finally, Maier and Seligman criticize (p. 28) motor response theories by stating correctly that they have difficulty dealing with situations that do not employ electric shocks or other noxious unconditioned stimuli. They particularly note the appetitive analogues in animals or the learned helplessness effect in the human subjects which are induced by exposure to loud noises. These comments also apply to the model under consideration.

The above type of criticism is believed unfair. Incompatable motor theories were developed to be applied to certain types of data. They were never presented as a general model of behavior. As Maier and Seligman themselves stress (p. 13), learned helplessness effects with appetitive analogues are subject to alternative explanation. This is believed also to be true for the human data presented. An extension of an Amsel frustration-type model to the human helplessness literature may prove to be a worthwhile endeavor. Studies like that of Glass and Singer (1972) cited by Maier and Seligman (p. 18) as supporting learned helplessness theory can more easily be interpreted by appealing to motivational variables such as those produced by demand characteristics.

It should not be construed from the above that the human data provided by Maier and Seligman are not important for the support of their theory. On the contrary the outgrowth of human experimentation on this topic is one of the most important contributions of the learned helplessness position. This writer's real interests are in these data and especially their importance for clinical application. However, comments on this area must serve the topic of another article.

In closing, it should again be noted that the critical stance adopted by this writer is not intended to attenuate the importance of the learned helplessness position. Many of the criticisms offered can be applied easily to the competing S–R positions. My purpose is to heighten the controversy so that both S–R and cognitive theorists will attend to their model's weaknesses. Clearly, the helplessness theory is in its infancy and needs time for development. Resolution of the helplessness controversy is believed to have important implications for the field of psychology well beyond the subject matter currently under discussion. The theory which becomes the most explicit in terms of operational specificity and predictive power, in the last analysis, probably will be the theory that loses. But at the same time, it probably will be the theory which is most responsible for advancing the field of psychology.

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On the Self-Regulation of the Occipital Alpha Rhythm: Control Strategies, States of Consciousness, and the Role of Physiological Feedback

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SUMMARY

In an experiment investigating the self-control of occipital alpha strength, a factorial design was employed with two levels of feedback (available or non-available), three levels of instruction (Cognitive, Oculomotor, and none), and two levels of lighting (on and off), except that the no-feedback-no-instruction group was not run. The Cognitive instructions were based on the strategies reported in the literature: To generate alpha, persons were instructed to relax, "let go," try to feel pleasant and serene, and allow themselves to review pleasant personal experiences; to suppress alpha, persons were instructed to concentrate, try hard, exert themselves mentally, and try to feel anxious or frustrated. The Oculomotor instructions to generate alpha were to visually blur and not focus, and to suppress alpha were to "look" and focus.

The results led to the following conclusions: (a) Feedback-augmented enhancement and suppression of occipital alpha strength is always mediated by learned control of oculomotor processes, although sometimes persons are not aware that they are employing this strategy, especially in the case of alpha enhancement; alpha feedback trainees learn to "not look" in order to enhance alpha strength. (b) Whenever Cognitive strategies are successful in producing alpha strength changes, this is due to the fact that they can be efficient mediators of changes in oculomotor processes. (c) Feedback together with simple Oculomotor instructions leads to more successful alpha control than either alone. (d) High or enhanced levels of occipital alpha strength are not invariably accompanied by the "alpha experience;" in fact, if a person is not led to expect it, the alpha experience will usually not occur during occipital alpha enhancement feedback. (e) States of nonsensory awareness, such as an absorption in thoughts and feelings, *are* reliably associated with enhanced occipital alpha strength. This association is what would be expected: Since vision is the dominant sensory modality in humans, it is reasonable that when visual processing is reduced during alpha enhancement feedback that there is also reduced sensory awareness in general and often greater attention to thoughts and feelings. (f) Alpha feedback per se is neither necessary for nor especially facilitative of the achievement of the alpha experience. Thus, *if* the alpha experience is more likely to occur with alpha feedback than without it, this is due to the special circumstances that make up the feedback *situation* and not due to alpha feedback per se.

Ever since Joe Kamiya announced in 1962 that his research participants were able to discriminate the presence and absence of the occipital alpha rhythm (8 to 13 Hz) in their electroencephalograms (EEGs), and that they could learn to suppress and enhance the amount of alpha when receiving continuous feedback information concerning their recent brain wave activity, there has been much controversy over the following points: (a) What are the strategies used by persons to achieve "alpha control"? How is it that these strategies result in alpha control? Which strategies are more efficacious, and why? (b) Are there any mood states or states of consciousness that are invariably or reliably associated with the suppression and/or enhancement of occipital alpha activity? If so, which ones, and how can we explicate such a relationship between psychological state and physiological process? (c) To what extent does physiological feedback *itself* contribute to the learning of alpha control when research participants are also instructed in an alpha control strategy? And (d) what measures constitute an adequate and reliable evaluation of the acquisition of alpha control, in the first place?

This article will begin with a review of the concepts and empirical findings that bear upon the above four issues.

Occipital Alpha Control Strategies

In reviewing the literature, we can separate into five categories the types of strategies that research participants and theorists have claimed to be instrumental in the self-control of the occipital alpha rhythm. These are strategies concerned with (a) attention, (b) the oculomotor system, (c) the degree of arousal, (d) somatic processes other than the oculomotor system, and (e) cognition/emotion.

Attention

Sometimes it is claimed that the strength of the occipital alpha rhythm is suppressed by increased attention and enhanced by decreased attention. For example, Lynch and Paskewitz (1971) suggest that

> alpha activity occurs in the feedback situation when an individual ceases to pay attention to any of a number of stimuli which normally block this activity. These stimuli may be cognitive, somatic, emotional, or anything, in fact, which will lead to alpha blocking. (p. 213)

It has long been known that ongoing alpha activity will usually block when an awake individual is presented, for the first time, with almost any type of environmental stimuli (Adrian & Mathews, 1934; Berger, 1930). Other researchers have noted a reduction in alpha activity during the performance of a mental task (Chapman, Armington, & Bragdon, 1962; Glass, 1964, 1967; Lorens & Darrow, 1962). However, alpha *enhancement* has been noted during prolonged visual attention sets (Williams, 1940), and there are many situations in which stimulus-induced alpha *enhancement* has been observed (Morrell, 1966). It turns out that the term *attention* is quite ambiguous. Mulholland and Runnals (1962), who also noted increases in alpha during increased "attention," concluded that

> the term attention can refer to neither a qualitatively consistent behavioral nor neurophysiological entity and that the classical and familiar alpha-attention hypothesis [decreased alpha with increased attention] refers to a special case, i.e., transitory alerting to an external signal. (p. 329)

This line of reasoning is supported by several biofeedback studies which have noted that alpha can be enhanced while a person maintains constant "attention" (Brown, 1970; Dewan, 1967; Mulholland, 1968; Peper, 1970). For example, when Mulholland (1968) was his own subject, he could easily maintain alpha even when alpha turned on a high-information auditory feedback (a radio program) presented over earphones.

It now appears that the alpha blockage that accompanies the "transitory alerting to an external signal" (i.e., the orienting response) is a function of oculomotor activity (Mulholland & Evans, 1966; see below). That is, alpha is often blocked when a per-

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son "pays attention" to a stimulus (presented to *any* modality) because oculomotor activity (e.g., eye-positioning movements) is often an "automatic" component of the general orienting response (Zickmund, 1969), and oculomotor functioning leads to alpha blockage. However, "paying attention" has no effect on alpha levels when the oculomotor system is nonoperative. Mulholland and Evans conclude:

> A search of most of the literature concerning human "attention" and the EEG does not reveal any experiment in which oculomotor functions have been excluded as a cause of the EEG phenomenon supposed to be associated with "attention". . . if there is any effect of "attention," it is on oculomotor functions which in turn cause a modification of the alpha-activation cycle. (p. 1279)

The Oculomotor System

The close association between vision and the occipital alpha rhythm has been recognized for quite some time (Adrian, 1943; Adrian & Mathews, 1934; Berger, 1930; Durup & Fessard, 1935). In almost all persons, alpha strength when the eyes are closed is significantly greater than when the eyes are open (e.g., Brown, 1970; Lindsley, 1960; Kamiya, Note 1). Furthermore, Nowlis and Kamiya (1970), Peper (1970), and Paskewitz and Orne (1973) have noted that the learning of alpha control is considerably easier with eyes open in a lighted room than with eyes closed or in a dark room. For example, Paskewitz and Orne found no increases in alpha strength across 60 trials when auditory feedback was presented to individuals in total darkness (with their eyes open). However, a second group run under the same conditions with a mere addition of low-level ambient light did show "lawful increases in alpha densities." Furthermore, the original group, who failed to show increases in total darkness, also increased densities in the presence of light. Peper (1970) hypothesized that individuals learn to "look" to suppress alpha and to "not look" to enhance it. This is much more difficult in the dark since there are no cues from the environment as to whether or not you are looking.

Extreme eye position has also been implicated as a means of enhancing alpha activity (Dewan, 1967; Mulholland, 1968). In Dewan's study, his research participants could quite successfully increase alpha by turning their eyes to an extreme upward position. To suppress alpha, they either opened their eyes and fixated on a nearby object or else kept their eyes closed and fixated them on an imaginary point in front of the eyes. However, when Dewan was his own subject, he found that mere elevation did not result in alpha activity if he also focused and converged his eyes in that position; *accommodation and convergence* had to be avoided to prevent blocking response. This observation allows us to understand Fenwick's (1966) and Budzynski's (1967) findings that some persons' alpha activity is not significantly related to eye position. It also resolves the suggested paradox of Kamiya's (Note 2) observation that persons can control alpha with the eyes in either an up or a down position, and of Brown's (1970) report that her subjects could enhance alpha with eyes open and without eye movements.

Research by Pollen (Note 3) suggested that *efferent* oculomotor functions—specifically, neural commands for accommodation—play an important role in the blocking of the alpha rhythm. Pollen inactivated the accommodation system with a drug and set up an artificial lens system to accommodate for the subject, who was able to produce alpha while reading text, which normally blocks this activity, supposedly due to accommodation.

However, it could not be the case that efferent commands for accommodation are the only factor in alpha blockage, since Dewan (1967) had reported that when he employed a drug which prevents accommodation, there was no effect on the ability of a research participant to control his alpha rhythm. Mulholland (1968) also reported evidence that it must be more than accommodation that is involved in alpha blocking.

Mulholland (1968), therefore, hypothesized that there were, in fact, *three* oculomotor processes involved in the blocking of the alpha rhythm: pursuit tracking movements, convergence movements, and lens accommodation. Mulholland referred to these three processes together as constituting the "eye-positioning control system," and suggested that while any one of these processes were active, the eye-positioning control system would be active to some extent, and therefore the alpha rhythm would be suppressed. To test this hypothesis, Mulholland had his research participants look at a stationary or moving target under conditions of (a) tracking the target with focused accommodation; (b) tracking with relaxed accommodation (blur tracking); and (c) no tracking with relaxed accommodation (blur no-tracking). He found most alpha during blur no-tracking, intermediate levels for blur tracking, and least during focused tracking. This result seemed to represent a series of conditions which have decreasing amounts of eye-positioning control, thereby confirming the hypothesis.

Peper (1970) also reported several elegant demonstrations of the role of oculomotor processes in alpha blockage and activation. In one experiment, he had persons attempt to track a moving target, the visibility of which was controlled by the presence or absence of the individual's alpha activity. When the feedback loop was set up so that the target appeared only during alpha, regenerative feedback developed in which there was a constant oscillation between alpha and no-alpha, demonstrating that whenever the person began to track, his alpha desynchronized. On the other hand, when the loop was set up so that the target only appeared during no-alpha, a runaway positive feedback system developed in which as soon as the alpha spontaneously desynchronized, the target would appear and the person would keep alpha off as long as he continuously tracked the target. As soon as his attention to tracking wavered, alpha would appear, the target would disappear, and the person had to motivate himself to track again. These findings strongly suggest that efferent oculomotor commands cause desynchrony.

Mulholland and Peper (1971) concluded that

> the most general hypothesis to account for the results reported here is that the combined processes of pursuit tracking, convergence, and lens accommodation will reliably block or markedly attenuate the alpha rhythm. Changes in any of these visual processes, however, which are not integrated with changes in the others in a way that optimizes vision are not necessarily associated with recordable decreases in the alpha rhythm; e.g., extreme ocular deviation may be associated with abundant alpha . . . . The reason that relationships between visual control processes and the parietal-occipital EEG are general—not specific—is straightforward. Visual control processes are occurring extensively in the parietal-occipital cortex. Because of the gross nature of EEG only extensive, large-scale changes of cortical activity associated with extensive visual processes can be recorded. On this level of observation there are reliable correlations between changes in visual control processes and the parietal-occipital EEG. . . . [Thus] autoregulation of the alpha rhythm by alpha feedback training is likely to be mediated by learned control of oculomotor and lens adjustment processes. However, the learner may not be aware of the mediating process. (pp. 573-574)

In addition to the studies cited above that demonstrate the relation between oculomotor functions and the alpha-activation cycle, there is some neurophysiological evidence that is quite consistent with this interpretation. Most obviously significant is that the oculomotor system has extensive representations in Brain Regions 17, 18, and 19, which include the parietal and occipital cortex (Robinson, 1968), the sites from which the EEG is recorded during almost all alpha feedback research. Also, the oculomotor pathway is located in the same zone of the brainstem tegmentum as the reticular activating system, and stimulation that can cause cortical desynchronization also causes alterations in eye movements (Bender & Shanzer, 1964).

In conclusion, there is very little doubt that there are some very close links between the occipital alpha rhythm and oculomotor processes. However, the important issue here is whether or not there are universally effective methods of controlling alpha strength that are *independent* of—or coextensive with—oculomotor processes. For example, Peper (1971) has suggested that oculomotor commands may be just a precondition for enhanced alpha levels. This possibility and others will be considered further at the end of this section on alpha control strategies.

Degree of Arousal

Many researchers have suggested that the strength of occipital alpha is a function of the level of arousal. This relationship is generally formulated by saying that alpha activity will be diminished if a person either becomes too aroused or too drowsy. Alpha appears to be maximal at an intermediate arousal level (Lindsley, 1956), namely, "relaxed wakefulness."

In order to adequately understand the connection between *alpha* and *arousal*, we must first take a closer look at these two terms.

The alpha rhythm is a synchronous brain wave with a somewhat arbitrarily defined frequency band of 8 to 13 Hz. There are at least two dimensions along which brain rhythms may be said to vary. The first may be called *strength* and roughly refers to the amplitude of the brain wave activity. The second is the *frequency* of the rhythm.

As the amplitude of the alpha rhythm decreases, we may speak of a continuous diminishing of alpha strength until there is no longer a synchronous rhythm within the alpha frequency band, at which point we describe the brain wave activity as *beta*—nonsynchronous, mixed-frequency, low-amplitude brain waves. However, as the frequency of the alpha rhythm decreases, the strength (amplitude) of alpha does not necessarily change, until the frequency falls below 8 cycles per second at which point we no longer speak of *any* alpha activity or *any* alpha strength. So when we say that alpha has disappeared, we may mean that it has been succeeded by either beta waves or theta waves (4 to 8 Hz), which are two different phenomena.

In the previous section on the oculomotor system, when "alpha blocking" was spoken of, we were referring to decreases in the *strength* of the synchronous alpha rhythm, resulting in the appearance of nonsynchronous beta activity. On the other hand, when people speak of the connection between alpha and arousal, they are most often referring to decreases in brain wave *frequency* that accompany changes in arousal along the sleep–wakefulness continuum. For instance, when a person is asleep or drowsy we usually find that his dominant brain wave frequency is somewhere between ½ and 8 cycles per second. On the other hand, when the person is wide awake, we usually find either the high-voltage alpha rhythm or the low-voltage beta activity with randomly mixed frequencies, mostly above 13 Hz.

The problem arises when theorists associate alpha with "relaxed wakefulness" and beta with "hyperarousal." Making such an association seems to confuse two types of "arousal." We may call these *behavioral arousal* and *oculomotor arousal*. Behavioral arousal refers to the sleep–wakefulness continuum, which is associated with changes in the dominant brain wave *frequency*, while oculomotor arousal refers to an activation of the oculomotor system which is associated with changes in the *strength* (amplitude) of the alpha rhythm with the lower limit being nonsynchronous beta activity.

By combining what we know about oculomotor research and sleep research, we can say that occipital alpha is maximal in a given individual when behavioral arousal is maximal (awake) and oculomotor arousal minimal (no oculomotor processing); and occipital alpha strength decreases when oculomotor arousal increases, until the brain wave activity desynchronizes. Many researchers have spoken of increases in behavioral arousal (viz., "hyperarousal") as associated with alpha desynchronization. However, we now understand that alpha desynchronization is *primarily* associated with oculomotor activation, often as a part of an orienting response, whether voluntary or involuntary. Furthermore, alpha desynchronization is associated with oculomotor activation whether or not the person also experiences a concomitant increase in behavioral arousal. Thus, the hypothesis that hyperarousal is an effective strategy for alpha suppression may, in effect, be reduced to one aspect of the previously presented oculomotor hypothesis.

On the other hand, it seems that "becoming drowsy," which is not necessarily associated with changes in relevant oculomotor processes, *is* an effective strategy for alpha suppression, since the dominant brain wave frequency falls below the alpha band as a person begins to fall asleep. This corresponds to the findings of Peper and Mulholland (1970) who reported that some of their subjects became drowsy in order to suppress alpha, and became tense or alert to increase it. However, it is important to note that in Peper and Mulholland's study, the research participants sat in totally dark rooms with their eyes closed. In such a situation, it is relatively easy to become sleepy or drowsy and thereby to decrease predominant brain wave frequency, which eventually would be read by an alpha wave monitor as "decreased alpha." However, reports of becoming drowsy to suppress alpha are very infrequent in studies in which participants' eyes are kept open (regardless of the presence of light in the room). Thus, although arousal level reduction (drowsiness) is a potential factor in alpha suppression, this variable can be eliminated (and *is* eliminated in the present research) by instructing research participants to keep their eyes open. Moreover, since it has been seen that the hyperarousal hypothesis may be reduced to one aspect of the oculomotor hypothesis, we are in the position to ignore the degree of arousal as a likely alpha control strategy in the research to be presented here.

Somatic Processes

Next we may consider whether there are somatic systems (other than the oculomotor system) involved in the mediation of alpha activity in the feedback situation. This possibility arises since changes in respiration rate, tidal volume, O_2 consumption, CO_2 elimination, arterial blood pH, blood lactate concentration and skin resistance have been reported to accompany the occurrence of more alpha activity during meditative states (Bagchi & Wenger, 1971; Kasamatsu & Hirai, 1969; Wallace & Benson, 1972). Watanabe, Shapiro, and Schwartz (1972) claim that self-induced anoxia is a major factor in the production of alpha waves during meditation. However, studies cited below seem to rule out this factor as an explanation of alpha changes in the feedback situation. Moreover, it is quite possible that the increased alpha levels during anoxia, as well as meditation, are due to decreased oculomotor processing. Watanabe et al. themselves noted that "impairment of visual functioning occurs in both anoxia and meditation"!

At any rate, there has been some research directly relevant to the question of somatic mediation in the alpha feedback situation. Nowlis and Kamiya (1970) asked five of their research participants who were particularly successful in learning control over alpha to increase and decrease alpha while their heart rate, blood pressure, breathing, and galvanic skin response were being monitored. No striking relationships were found between alpha and these other measures. Beatty and Kornfeld (1972) recorded heart and respiration rates while 14 persons were trained to produce occipital alpha and beta frequency activity differentially. These individuals showed discriminant control of the EEG frequencies, but no significant changes were found in either heart or respiratory systems. Also, in a study investigating alpha and heart rate control, Schwartz, Shaw, and Shapiro (1972) concluded that the mechanisms of alpha and heart rate control are different.

In sum, there is no evidence to date that somatic processes other than oculomotor functions serve as mediators of the self-control of occipital alpha.

Cognition/Emotion

The most exciting and controversial aspect of the self-control of occipital alpha has centered around the claim that persons learn to manipulate their state of consciousness or mood state when learning to enhance and suppress the occipital alpha rhythm. It has been proposed that cortical alpha activity is related to a psychological state very similar to that which occurs during Zen or Yoga meditation, and that individuals in the alpha feedback situation learn to produce this state (Brown, 1970, 1971; Kamiya, 1968, 1969). In Brown's (1970) study, in which the participants' task was only to *increase* alpha by means of visual feedback (eyes open), the most popular control strategy was "concentrating, or mental imagery." Also reported were "feeling relaxed by the experience" and "actively attempting to relax." In the eyes-closed condition of Nowlis and Kamiya's (1970) study, the following methods of sustaining alpha were reported (in order of decreasing popularity): relaxation, awareness of inhalations and exhalations, letting go, floating, feelings of pleasure and security, sensual warmth, not focusing, and awareness "in back." To keep alpha off, the tactics were the following: being alert and vigilant, tension and agitation, attitude of "holding on," and imagining seeing the room. Brown's (1971) reported methods for increasing alpha were mostly consistent with those listed above: in addition were reports of "increased awareness of thoughts and feelings" and "reviewing personal experience." Kamiya (1968) reported that his research participants increased alpha by developing a "noncritical" attitude about the external environment, in general, and the experiment in particular; they calmed their minds, relaxed their mental apparatus, stopped thinking about the outside world and just let themselves be "carried along by the tone."

Additional discussions of cognitive/emotional strategies of alpha control (henceforth referred to as simply *Cognitive strategies*) will be included in the following sections of the introduction.

Summary

The two strategies that seem most likely to be employed in the self-control of occipital alpha are the ones I have called the Oculomotor and Cognitive strategies. At present the strongest and best supported hypothesis is the Oculomotor hypothesis, which claims that a person in an occipital alpha feedback experiment learns to control oculomotor functions which can facilitate or inhibit the occurrence of alpha in the occipital EEG. Also still viable is the Cognitive hypothesis which claims that occipital alpha can be controlled by means of voluntary changes in one's state of consciousness or mood state. The major possibility to consider here is that there are certain Cognitive strategies of alpha control that are *independent* of Oculomotor strategies. Perhaps relaxation of the oculomotor system, which allows for initial alpha enhancement, is a precondition for the attainment of a certain altered state of consciousness (the "alpha experience") which is associated with even greater enhancement of the alpha rhythm (Peper, 1971). Perhaps if research participants are instructed in the appropriate Cognitive strategy to use—instructions based on the literature and pilot studies—they will be able to control the occipital alpha rhythm more successfully than persons who receive no instructions or even persons instructed in the Oculomotor strategy. It would be of particular interest if persons instructed in this Cognitive strategy could generate alpha strength significantly higher than persons in the other conditions, and could do so independently of changes in the oculomotor system.

Although oculomotor relaxation and activation may be an efficient means for enhancing and suppressing alpha activity, respectively, the alpha levels seen under "enhance" conditions when the "nonlooking" strategy is employed have rarely increased above those seen when a person simply closes his eyes and relaxes (Lynch & Paskewitz, 1972). However, perhaps those persons who are able to achieve a more general and widespread relaxation of the "mental apparatus" (Kamiya, 1969), perhaps related to a quasi-meditative state of consciousness, *will* be able to enhance alpha levels significantly above eyes-closed levels.

In sum, the question of primary interest now concerns the relation of the Cognitive strategy to the control of the alpha rhythm. The fact that oculomotor functions play an important role in the activation and attenuation of alpha has been established beyond question. Therefore, it is now in order to ask what are the possible states of affairs concerning the relation between the Oculomotor and Cognitive strategies of alpha control. These are listed below. Please note that "universal" is meant to be read as "for nearly all physiologically intact human adults."

1. There are *no* universal Cognitive strategies (at least in the form used in this research, which are based on those most frequently reported in the literature) that are related to occipital alpha control in a consistent manner; only certain Oculomotor strategies are universal, consistent, and successful.
2. There are both certain Cognitive and certain Oculomotor strategies that are universal, consistent, and successful in producing occipital alpha strength changes, but Cognitive strategies are effective only to the extent that they successfully mediate changes in oculomotor functions.
3. There are both certain Cognitive and certain Oculomotor strategies that are universal, consistent, and successful in producing alpha strength changes, *and* they are relatively independent of each other.

One of the major motivations for conducting the research presented in this article was to gather data that would allow us to decide among these three hypotheses.

Occipital Alpha and States of Consciousness

The aspect of alpha feedback training that has created the greatest general interest is the popular belief that this training induces a quasi-meditational state of consciousness, known as the "alpha state" or the "alpha experience," by increasing the strength of the EEG alpha rhythm. Frequently cited in support of this view is the fact that the EEGs of Yoga students, Zen masters, and American meditation students often show increased alpha strength during meditation (Anand, Chhina, & Singh, 1961; Kasamatsu & Hirai, 1966; Wallace, 1970).

In addition to positing such an association between high alpha strength and the "alpha state," researchers have suggested a corresponding relationship between the occurrence of beta activity and a "beta" state of consciousness.

Some of the reported subjective states associated with changes in alpha strength in the feedback situation have already been mentioned under the section on Cognitive/Emotional strategies of alpha control. In general, it has been widely reported that people find that alpha enhancement condition to be a pleasant and relaxing experience; feelings of bodily lightness, time perception distortion, mental relaxation, and obliviousness to the external environment are often reported (Brown, 1970; Kamiya, 1968, 1969; Nowlis & Kamiya, 1970; Kamiya, Note 1; Hart, Note 4). Brown (1970) found that persons "who lost awareness of all environmental factors except the light, or who felt dissolved into the environment tended to have higher levels of alpha abundance" (p. 450). Stoyva (1973) has reported that feedback training in alpha "generally results in a shift away from the thinking of ordinary wakefulness—the subject is more immersed in his internal world, feels relaxed, is in a nonstriving, 'letting go' mode" (p. 388).

Walsh (1974) used the following subjective dimensions to identify the alpha related state:

> transitional state between sleep and complete alertness; reduced body awareness; relaxation; floating; lightness; neutral to pleasant emotions; content, peaceful, slightly high; thoughts undirected or absent; free-flowing thought or thoughtlessness. (p. 432)

In addition, Walsh reported the following subjective dimensions to be characteristic of the nonalpha-related state:

> either asleep or highly alert; heightened body awareness; tension; uncomfortable physical sensations; unpleasant emotions or emotional excitement; fear; annoyance, anxiety; directed thought; problem solving; intense concentration. (p. 432)

However, some observations that have been made by almost every EEG-researcher both in and out of biofeedback research, suggest that there is no simple and direct relationship between alpha strength and mood states or states of consciousness. In almost all persons, there is a striking difference in alpha levels between eyes-open and eyes-closed–resting conditions. Yet no significant changes in mood or state of consciousness have been related to simply closing the eyes. However, in a previous section, we have already discussed the possibility that subjective state changes are associated with (further) alpha changes independent of alpha changes caused by oculomotor alterations.

Although consistent relationships have often been reported between alpha strength and subjective experiences in biofeedback experiments, it is important to note that frequently the experimental setting and procedures, the course of the experiment, and possible "demand characteristics" (Orne, 1962) will considerably influence subjective reports. Those researchers who *have* found consistent relationships between alpha strength and subjective states, have not adequately controlled for the effects of explicit or implicit suggestions to the research participant as to how the "high alpha state" or the "beta state" will "feel." There is also the problem that some of the positive affect in the situation may be a function of the person's feeling of success at the task. It is possible, then, that the experience of the alpha state will vary greatly among individuals depending on set, motivation, preparation, expectation, constitution, and personal biases and preferences.

In sum, although certain subjective reports seem to be consistently associated with changes in occipital alpha strength during feedback training, based on the available research data we cannot rule out special characteristics of the experimental situation which may have influenced the findings. Therefore, the present research was designed to include as investigation of the effects of suggestion or "demand characteristics" on the subjective states reported during the occipital alpha feedback situation.

The Role of Physiological Feedback

Since the discovery of the phenomena of the self-control of the alpha rhythm, it has been assumed that physiological feedback itself is necessary for people to learn selective control of aspects of their EEG. However, might there be other ways for people to gain such control just as fast and easily without feedback? Does the control provided by the operant conditioning of alpha activity differ from that obtained by other methods for controlling neural processes? For example, does the administration of task instructions produce effects similar to feedback? If so, what *kinds* of instructions?

These questions received no explicit empirical investigation until Beatty's (1972) study. Beatty reported that his research participants were able to systematically modify their occipital alpha densities in either of the following two conditions: (a) when provided with information giving appropriate strategies for producing such changes and (b) when given only second-by-second feedback about their success in achieving the response criteria. Individuals provided with both prior information and feedback were no more successful than those given either alone.

The prior information which Beatty gave his research participants is similar to the type of cognitive/emotional strategies that were discussed in the previous section. Specifically, they were told the following:

> (a) Beta activity is related to the orienting response to novel stimuli, sensory or mental attentiveness, tension, aggravation, and frustration.
>
> (b) Alpha rhythms are often said to be associated with feelings of calmness, pleasant relaxation, and increased inner awareness. However, some naive subjects report very different feeling states, such as sadness, while producing alpha rhythm.
>
> (c) Relaxation and regular breathing sometimes facilitate production of alpha waves. (p. 154)

Since Beatty's findings raise some obviously serious issues for biofeedback in general and for occipital alpha feedback in particular, I decided to reinvestigate the role of feedback itself under a more extensive design. It is important to note that there are actually two issues here:

1. Can persons who are merely instructed in alpha control strategies produce increases

(or decreases) in alpha strength significantly above (or below) resting levels? If so, can they accomplish this with both the Oculomotor strategy and with the Cognitive strategy or with just one or the other?

2. If the first question is answered affirmatively, are those persons who are merely informed as to the nature of the task *equally* successful as those who receive feedback or both instructions and feedback?

Measuring the Degree of Acquired Control

What constitutes a meaningful measure of response acquisition ("alpha control") in the first place? It has been suggested, for instance, that to demonstrate conclusively an enhancement of alpha, there must be an increase above the level observed during a baseline period (Katkin & Murray, 1968). (A baseline period is usually one in which the individual is told to "rest" and does not receive any information feedback on his alpha strength.) If alpha levels during enhance periods do not rise above the levels during baseline periods, must we not conclude that any increases across trials in alpha strength were due to spontaneous changes independent of any learning due to feedback? This is the question that we ask, for example, in connection with the results of Kamiya's (1969) study, in which he alternated 2½-min blocks of enhance alpha, rest, and suppress alpha. The "suppress alpha" blocks showed a clear downward trend in alpha strength, and there was an even more striking increase across trials in the enhance alpha blocks. However, the resting, "baseline" curve increased just as fast as the "enhance" curve, and was, in fact, slightly higher. No one would doubt that Kamiya's subjects showed a *discrimination* between enhance and suppress conditions, but it is questionable as to whether or not it was demonstrated that they really learned to *increase* their absolute alpha levels. Kamiya's interpretation was that the baseline increased because his research participants much preferred the "tone-on state" over the "tone-off state" and therefore waited in this preferred mode. Since this interpretation is not entirely convincing, we should adhere to the following two methodological points: (a) alpha enhancement (or suppression) can only be conclusively demonstrated in relation to increases (or decreases) above (or below) a baseline, resting level, and (b) research participants should be carefully instructed not to practice either their suppress or enhance strategies during these baseline periods.

Another valid criticism has been made to those experiments in which only one baseline recording is taken at the beginning of the session. Almost any initial baseline recording may be contaminated by apprehensions about the forthcoming experiment and other such influences (Lynch & Paskewitz, 1971). Therefore, baseline measurements should be made repeatedly during the course of feedback training in order to control for natural changes in baseline levels, and in order to adequately determine whether or not (and to what extent) persons can show actual increases or decreases in alpha strength.

Another difficulty in evaluating alpha control centers around the issue of *optimal* baseline levels. For example, Lynch and Paskewitz (1971) have noted that

> although *S*'s can very quickly learn to suppress their alpha activity, often within the first trial, increases during instructions to augment alpha densities are far more difficult to achieve, and rarely rise above alpha levels which naturally occur under optimal conditions. (p. 211)

("Optimal conditions" are invariably considered to be those when an individual is awake, relaxed, with eyes closed, or with eyes open and no light.) Thus, in order to determine whether or not persons in the alpha feedback situation can enhance alpha above optimal resting levels, the experiment must include frequent baseline measurements made under "optimal resting conditions." This was simply accomplished in the present research by running each research participant through a feedback session in which the lights were entirely off throughout the session, including the frequent rest (baseline) periods.

In order to make reliable and meaningful evaluations of the level of "baseline alpha," there are many variables that must be taken into consideration. These variables can be divided into three classes, depending on whether they are (*a*) fixed at definite values that are arbitrary or determined by those aspects of the phenomenon that the researcher is interested in investigating, (*b*) fixed at definite values in order to insure nonarbitrary, stable baselines, or (*c*) independently varied. In all experimental reports concerning alpha feedback, for each variable that comes under the heading *a*, an associated statement should be included which acknowledges the limitations that are thereby placed upon the generality of the results to the question of the acquisition of alpha control.

I will discuss two *a* variables first and describe how they are specifically dealt with in the experiment to be reported here.

1. This research is concerned with the control of the alpha rhythm from the right occipital cortex, using a bipolar occipital-frontal recording; the results are not necessarily relevant to the control of the alpha rhythm from any other cortical location.
2. The property of the cortical response that is displayed is the strength (voltage) of the brain wave activity in the alpha (8 to 13 Hz) frequency band. This is slightly different from most other experiments in the field, which signal that alpha is either present or absent, based on some arbitrary amplitude criterion of what constitutes alpha; with these methods of signaling alpha, all waves between 8 and 13 Hz are counted as equal regardless of amplitude. On the other hand, the method used in the present research—a continuously varying signal of alpha *strength*—is sensitive to *both* the amplitude of the alpha rhythm and the presence versus absence of alpha. This method, although similar to the other methods, is more sensitive to changes in alpha activity, and it eliminates "ceiling effects."

Next are the variables whose values are fixed in such a manner as to produce nonarbitrary, stable baselines. (a) The modal quality which expresses the signal in this research is tone intensity. Since the feedback signal is continuous, varying in volume in a smooth manner, the problem of distracting or arousing the research participant by simple tone-on/tone-off schemes is eliminated. Thus, the danger of artificially lowering enhance alpha scores relative to baseline level is reduced. (b) The research participant's posture is upright, sitting in a comfortable straight-backed chair. This reduces the possibility of altered baselines due to drowsiness. (c) The length of the session and trials is kept moderate so as to minimize the likelihood of drowsiness and fatigue while still allowing ample time for learning. (d) Three types of trials are administered: suppress, generate, and rest. All are of equal length in order to insure that the average alpha strength for the different trial types will be comparable. (e) Research participants are instructed to keep their eyes open for all periods regardless of whether the lights are on or not. This reduces the likelihood of drowsiness. (f) Most importantly, in order to insure meaningful baseline scores, research participants are always asked to do anything they like during the rest periods except to practice producing or suppressing imaginary tones, and they are asked to refrain from making large body movements, which may disturb the recordings.

Finally, there is one variable that is relevant to stable baseline recordings that is independently manipulated in this research. This is the presence of light in the experimental room, which has by far the single greatest effect on baseline alpha strength. Therefore, each research participant is run through two sessions: one with lights off, and one with a low level of ambient lighting.

With the above procedures, we have made frequent baseline determinations that are reliable, meaningful, and reasonably stable across trials.

There remains the problem of how to best operationalize the dependent variable. The property of the cortical response that is measured (alpha strength) has already been discussed. The raw data is in the form of a single "absolute alpha" score (average amplitude in microvolts peak-to-peak) for each enhance, suppress, and rest period.

To measure alpha enhancement, we use the percent generate success score ("generate" and "enhance" are interchangeable), which is derived by the following equation: $(G_i - R_i)/R_i$ where $G =$ raw score for the generate trial, $R =$ raw score for the rest period, and $i =$ the trial number. Thus, each generate score is directly compared to the baseline measurement of the same number, which is never more than one period away. The percentage (ratio) form allows us to correct for the large individual differences in alpha strength.

Similarly, alpha suppression is measured by the percent suppress success score, which is derived by the following equation: $(R_i - S_i)/R_i$, where $S =$ raw score for the suppress period. Discrimination is measured by $(G_i - S_i)/R_i$. Increases and decreases relative to optimal baseline levels are measured by the percent success scores on those sessions during which the lights are off.

Summary

The present experiment was designed to gather data on the issues corresponding to the following questions:

1. What is the relationship between the Oculomotor and Cognitive strategies of occipital alpha control?

2. Are there any mood states or states of consciousness that are directly associated with alpha activity? What effect, if any, do demand characteristics have on the reports of experiences during alpha feedback?

3. To what extent does physiological feedback *itself* contribute to the learning of alpha control?

METHOD

Design

The basic design was a 2 × 3 × 2 × 3 × 6 mixed factorial, corresponding to the following independent variables, respectively: two between-subjects variables (Feedback and Instructions) and three within-subject variables (Lights, Trial Type, and Trial Numbers). However, the no-feedback–no-instruction group was not run. Two additional between-subjects variables were included but were not of primary interest. These were Session Order (light-dark or dark-light) and Sex.

The first between-subjects independent variable is whether or not research participants received auditory feedback (FB or NFB) as to their moment-to-moment occipital alpha strength. This variable had two major purposes. First, it allowed us to determine to what extent feedback *itself* helps persons learn alpha control when they are also informed as to nature of the task. Second, it allowed us to gauge the relative effectiveness of Oculomotor and Cognitive strategies uncontaminated by the effects of feedback itself. If it is true that the Cognitive strategy is in no way effective in producing alpha strength changes, even through mediation of oculomotor functions, we would expect the no-feedback–Cognitive-instruction group to show no significant success at alpha control.

The second between-subject independent variable is type of instructions—Cognitive (CI), Oculomotor (OI), or none (NI). This variable had two purposes. First, it allowed us to determine the relative efficiency of these types of control strategies. The no-feedback–no-instruction group was not run, but the feedback–no-instruction group allowed us to assess the effects of giving instructions on the development of alpha control during the experimental session. Second, the instruction variable allowed us to determine the effects of suggestion (instructions) on the reported subjective states accompanying high and low alpha strength. This was accomplished as follows: The Cognitive instructions implicitly suggested, to those persons who received them, what kinds of subjective feelings to expect in relation to increased and decreased alpha levels, whereas the persons in the no-instruction group were given no idea what to expect. A comparison of the written subjective reports of the subjects in these two groups, matched for the degree of acquired control of alpha, illustrates the effects of suggestion. In addition, we would expect only those aspects of subjective state that are present in a majority of the reports from the successful individuals in the no-instruction group to be reliable subjective correlates of increased or decreased alpha strength.

The first two variables—Feedback and Instructions—define the five major between-subjects cells, which are FB-CI, FB-OI, FB-NI, NFB-CI, and NFB-OI ($n = 8$ for these cells). Recall that the NFB-NI group was not run.

The first within-subjects independent variable is Lights. Each person was run through two sessions, one with lights on and one with lights off in the experimental room. The on condition is actually low ambient lighting, and the off condition is total darkness. If it is true that Cognitive and Oculomotor strategies are independent means of manipulating occipital alpha strength, we would expect that, although the degree of learning in the two groups would be similar with the lights on, the Cognitive group would do significantly better than the Oculomotor group under the lights-off condition. This is because the absence of cues from the external environment would make it very difficult for persons in the Oculomotor group to "look" and "not look," while this situation would not be as great a detriment to persons who were using Cognitive strategies of alpha control that are potentially independent of oculomotor processes. On the other hand, if it is true that control of the oculomotor functions, directly or indirectly, is the only way to affect alpha strength, we would expect persons to do significantly worse when the lights are out, regardless of the type of instructions they received.

The first controlled variable is order of trials within sessions. All six possible orderings of the three-trial sets were used once in each session with the same order for every session. This order was randomly picked with the stipulations that (a) the very first trial would be a generate trial, and (b) no two trials of the same type would occur consecutively. The obtained order was GRS RSG RGS GSR SGR SRG. This order assures that each type of trial occurs equally late in the experimental session, and that each type of trial follows each of the other two kinds equally often.

The second controlled variable is time of day. Each person was run at the same time of day for both sessions in order to minimize individual changes in motivation, arousal level, etc., between session days.

The measures of the first dependent variable, degree of acquired control of occipital alpha, were discussed in the introduction.

The second dependent variable, subjective report, was obtained through postsession questionnaires, which consisted of a number of items, including a list of methods used for increasing and for decreasing the volume of the feedback tone (or for entering the two types of subjective states for the NFB subjects).

Research Participants

Forty undergraduates from the University of Colorado served as research participants in order to partially satisfy the requirements of an introductory psychology course. They were informed as to the general nature of brain wave feedback, but no mention was ever made of alpha, beta, or any other type of brain wave, in order to guard against the contaminating effects of possible preconceived notions that persons may have obtained from popular press accounts of alpha feedback research.

To minimize the subject heterogeneity, the following restrictions were used in selection: (a) age range of 18 to 23; (b) no prior biofeedback experience; and (c) alpha strength of at least an average of 15 microvolts peak-to-peak per minute with eyes *closed,* since persons with less alpha than this rarely show any alpha control.

The research participants were randomly assigned to the five between-subjects conditions (n $=8$) with the stipulation that there were equal numbers of each sex in each condition.

Apparatus

The EEG was recorded by bipolar leads attached to the right occipital and right frontal lobes (O_2 and F_4 of the 10–20 system) with the left mastoid as ground. This electrode placement was chosen since it generally gave us the occipital alpha recording of highest amplitude, and it is one of the two placements used by Nowlis and Kamiya (1970), whose research we were interested in replicating. Although other occipital alpha researchers have used different placements, we know of no reports of any relevant differences in results when using the O_2–F_4 placement versus other occipital placements such as O_1–O_2, O_z–ear, or O_2–C_4 (e.g., Nowlis & Kamiya, 1970).

The EEG was amplified and filtered by a Biofeedback Systems, Inc., Model AT-1. The center frequency of the alpha filter on this unit is 10.0 Hz, with a 3-db. attenuation of 7.5–8.5 Hz and of 11.5–12.5 Hz, and with a 20-db. attenuation of 5.0–7.0 Hz and 15.0–17.0 Hz. The output of the AT-1 was fed to two locations: an audio feedback generator and a digital quantifier.

The audio feedback was in the form of an intensity-modulated tone appearing over a headphone set. The intensity (volume) of the tone is essentially proportional to the strength of the alpha signal being sensed.

The digital quantifier, Model DQ-1, also manufactured by Bio-feedback Systems, Inc., is a variable time base, four-digit accumulating counter. The integration time was set to automatically stop every 60 sec, and to reset after about 2 sec. The count accumulated over the 60-sec time period is proportional to the area under the curve of alpha strength versus time.

The research participants sat in an upright, comfortable chair which was placed approximately in the center of a 9′ by 10′ sound-attenuated and carpeted room.

Procedure

After the recording electrodes were attached, the participant was seated in the experimental room and was read the initial instructions and orientation materials. First he was told that any of his preconceived notions about biofeedback training were likely *not* to be relevant to the present research. Next the basic theory of biofeedback was explained. Then the experimenter went back to the control room and gave the subject 5 min to relax, followed by two 3-min baseline recording periods (first with eyes open, then with eyes closed).

After the baseline recordings, the experimenter reentered the experimental room and read the participant the instructions explaining the nature of the task. Those in the FB conditions were instructed to increase the tone volume as high as they could during the "maximize" periods, to decrease it as low as they could during the "minimize" periods, and to not try to produce or suppress imaginary tones during the rest periods, at which times the feedback circuit would not be operating. Participants were warned against being satisfied with partially correct hypotheses—they were encouraged to continually search for better methods to increase and decrease the tone volume. They were also asked to keep their eyes open at all times and to sit up straight in their chair. By means of the intercom, the participants were informed of the nature of the next trial by the words "maximize," "minimize, " or "rest."

Participants in the NFB groups were not provided with the feedback signal (the tone), but were asked to wear the headphones. They were instructed in two types of subjective states that they should practice entering during the session when signaled through the intercom. The Cognitive instructions were similar to the control strategies reported in the literature: to generate alpha, participants were instructed to relax, "let go," become "noncritical," and allow themselves to review pleasant personal experiences. The high alpha state was described as one of "drifting tranquility, flowing restfulness, and well-being." To suppress alpha, they were instructed to concentrate, become "critical," try hard, and exert themselves mentally. The low alpha state was described as one of intense awareness and concentration, and sometimes anxiety or frustration. The Oculomotor instructions to generate were, basically, to blur and not focus, and to suppress were to "look" and focus. To eliminate the possible interference of preconceived notions, no mention was ever made to any person in any condition of alpha or beta rhythms, although I will use these terms here to simplify the presentation. The signal for generate alpha was "receptive state," and the signal for suppress alpha was "action state." During the rest periods, the persons in the NFB groups were instructed not to practice entering either of the two subjective states.

Persons in the FB-CI and FB-OI groups received both the feedback instructions and the corresponding strategy instructions. Individuals in the FB-NI group received the feedback instructions but were given no idea as to which processes the feedback tone might be associated. It was emphasized to all participants that they should keep their eyes open at all times regardless of whether or not the lights were on.

Research participants were then run for 18 experimental trials of 3-min duration, which were divided into six blocks of three trials each, as described previously. The absolute alpha strength for each trial was recorded.

At the end of the session, participants were brought back into the control room where they were unwired and asked to fill out the questionnaire.

RESULTS

Degree of Acquired Control

The basic analysis of the data was accomplished by two types of analysis of variance (ANOVA) and the three types of dependent variables, giving six F tables. ANOVA 1 refers to the analyses in which the FB-NI group was left out. The major between-groups cells in these analyses are FB-CI, FB-OI, NFB-CI, and NFB-OI, representing a 2 × 2 factorial (Feedback × Instructions). ANOVA 2 refers to the analyses in which the NFB groups were left out to leave the major between-groups cells of FB-CI, FB-NI, and FB-OI. These three cells represent one independent variable with three levels (of Instruction).

Since they allow one to cancel out the large between-subjects differences in alpha strength, the percent success and discrimination scores are much more sensitive than the absolute alpha strength scores. Therefore, I chose an alpha level of .05 for the former two ANOVAs and an alpha level of .02 for the latter.

Absolute Alpha Strength Scores

ANOVA 1. In discussing the results of this ANOVA, there will be no need (nor space) to report those effects which have corresponding effects (i.e., with identical interpretations) in the more sensitive percent success ANOVA.

As expected, overall alpha strength during the experimental session was higher when the lights were out (mean of 177 counts per minute [c/m]) than when the lights were on (144 c/m), as indicated by the main effect of Lights, $F(1, 16) = 13.91$, $p < .005$.

A highly significant main effect of Trial Type, $F(2, 32) = 30.25$, $p < .000005$, indicated that across all groups, sessions, and trials, generate levels were, in fact, above resting levels, which were above suppress levels.

Overall alpha strength showed a spontaneous increase over the first half of the session and then leveled off, as indicated by a highly significant main effect of Trial Number, $F(5, 80) = 17.32$, $p < .0000001$.

The alpha levels for the six trials were 149, 154, 159, 164, 163, and 163, respectively. Because of such effects it is important to include frequent baseline measurements in order to determine that alpha increases are actual increases and not merely due to spontaneous changes occurring during the feedback session.

ANOVA 2. The interpretation of the relevant effects from this analysis are similar or identical to the same effects from ANOVA 1.

Percent Success Scores

ANOVA 1. Our research participants were much more successful with the lights on than with the lights off (percent success means of 22.8 vs. 6.8) as indicated by the significant main effect of Lights, $F(1, 16) = 40.59$, $p < .0001$.

The main effect of Feedback, $F(1, 16) = 15.86$, was highly significant ($p < .002$), demonstrating that persons who received feedback were significantly more successful than those who did not (percent success means of 21.4 vs. 8.2). Moreover, the greater success of those who received feedback did not depend on the type of instructions received, as reflected in the nonsignificant Feedback × Instruction interaction, $F(1, 16) = .07$, $p > .78$. However, the effect of Feedback did depend on the Lights variable, as shown by a significant Light × Feedback interaction, $F(1, 16) = 5.96$, $p < .03$, which indicated that the superiority of feedback was significantly greater with the lights on, although persons did better with feedback in either case.

Persons who received Oculomotor instructions were significantly more successful than those who received Cognitive instructions (with percent success means of 18.8 and 10.8, respectively), as indicated by the main effect of Instructions, $F(1, 16) = 5.75$, $p < .05$. However, the significant Instruction × Light interaction, $F(1, 16) = 13.15$, $p < .003$, which is illustrated in Figure 1, shows that this effect of Instructions depended entirely on whether the lights were on or off. When the lights were *on*, persons who received Oculomotor instructions were much more successful than those who received Cognitive instructions (means of 31.4% vs. 14.3%). However, with the lights *off*, there was no significant difference between the two types of instructions, $F(1, 16) = .13$, $p > .5$.

There was no significant difference in percent success scores between the generate and suppress tasks (nonsignificant main effect of Task), regardless of the type of instructions received (nonsignificant Task × Instruction interaction), and regardless of whether or not feedback was available (nonsignificant Task × Feedback interaction). However, there was a significant Task × Light interaction, $F(1, 16) = 14.36$, $p < .002$, which demonstrated that the absence of light had a much greater detrimental effect on the generate task than on the suppress task, which was true regardless of the type

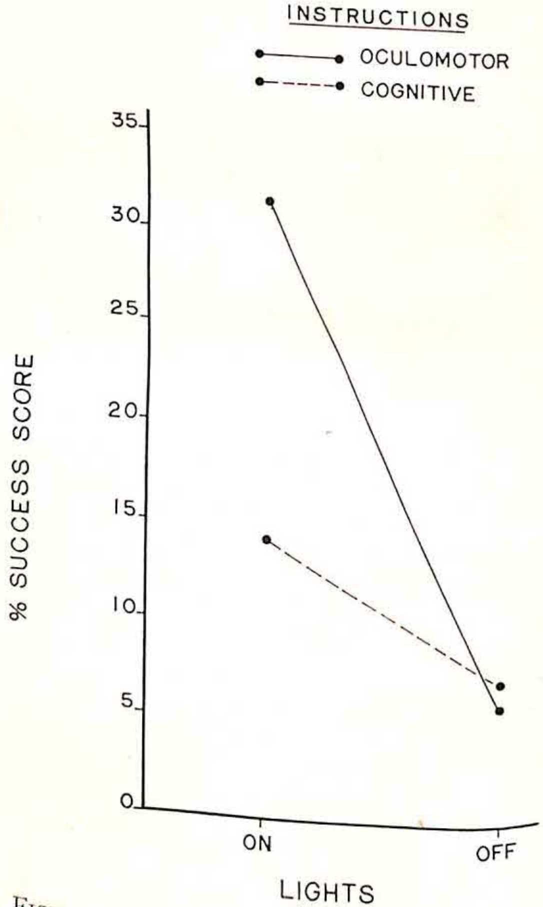

FIGURE 1. Light × Instruction interaction for percent success ANOVA 1.

TABLE 1

Means for Trial Number Effects from Percent Success Score ANOVA 1

Test	Trial No.					
	1	2	3	4	5	6
Trial No.	10.6	14.7	13.2	15.0	15.6	19.6
Trial No. × Feedback						
FB	14.3	19.8	19.9	23.0	24.0	27.3
NFB	6.9	9.6	6.5	7.1	7.3	11.8
Trial No. × Light						
On	12.7	23.8	20.8	23.3	24.8	31.6
Off	8.6	5.6	5.6	6.7	6.4	7.6
Trial No. × Task						
G	4.0	13.8	12.9	17.2	17.6	22.6
S	17.3	15.6	13.4	12.9	13.6	16.6

Note. ANOVA 1 = analyses of variance in which the feedback-no-instruction group was left out. FB = feedback, NFB = no feedback, G = generate, S = suppress.

of instructions received (nonsignificant Light × Task × Instruction interaction).

Persons who received feedback showed significant increases in success scores across trials, while those who did not receive feedback showed almost no increases in success across trials (see Table 1). This conclusion is supported by a significant main effect of Trial Number, $F(5, 80) = 6.39$, $p < .0002$, and a significant Trial Number × Feedback interaction, $F(5, 80), = 2.49$, $p < .05$. Moreover, persons showed significant increases in success scores across trials only when the lights were on, as shown in Table 1, and as indicated by the Trial Number × Light interaction, $F(5, 80) = 6.94$, $p < .0001$.

Finally, the generate success scores increased across trials while the suppress success scores did not, as indicated by the Trial Number × Task interaction, $F(5, 80) = 4.67$, $p < .002$ (see Table 1). Furthermore, the significant triple interaction between Trial Number, Lights, and Task, $F(5, 80) = 4.10$, $p < .005$, showed that this Trial Number by Task effect came mostly from the lights-on sessions, since success scores did not increase across trials for either task when the lights were out.

ANOVA 2. The most important results from this analysis is the fact that the main effect of Instructions, $F(2, 12) = 1.99$, $p > .17$, and the Light × Instruction interaction, $F(2, 12) = 3.81$, $p > .05$, did not reach significance, although the latter came very close. The percent success means for the Oculomotor, Cognitive, and no-instruction groups for lights-on and lights-off sessions are shown in Table 2. Two orthogonal comparisons were run on the interaction: (a) Lights by CI and OI versus NI, and (b) Lights by CI versus OI. Neither reached significance, but in both cases $.10 > p > .05$. Orthogonal comparisons on the main effect of Instructions (CI and OI versus NI, and CI versus OI) also did not reach significance, but again $.10 > p > .05$. However, a comparison between the success scores of the CI and OI groups during the lights-on sessions *was* significant, $F(1, 12) = 9.78$, $p < .01$. This suggests that with the lights on, the feedback subjects who received the Oculomotor instructions were the most successful, with approximately equal success for the Cognitive and no-instruction groups. With the lights off, there were no significant differences in performance.

Discrimination Scores

Separate ANOVAs are not necessary for the discrimination scores for the following reason. Recall that percent generate success $= (G_i - R_i)/R_i$ and that percent suppress success $= (R_i - S_i)/R_i$. The sum of these two success scores equals the discrimination score: $(G_i - R_i) + (R_i - S_i) = G_i - S_i$. Therefore, all the significant effects

TABLE 2

PERCENT SUCCESS MEANS FROM ANOVA 2

Instruction	Lights on	Lights off	*M*
Oculomotor	40.4	9.5	24.9
Cognitive	24.6	11.0	17.8
None	22.6	8.6	15.6

Note. ANOVA 2 = the analyses of variance in which the no-feedback groups were left out.

from the percent success ANOVAs are the same ones that are significant for the discrimination ANOVA, except for the effects that include the Task variable, which, of course, is not in the discrimination ANOVAs.

Special ANOVAs

Two additional partial-ANOVAs were of interest in order to compare groups which were not compared in ANOVAs 1 or 2. The first of these was an ANOVA consisting of the data from the following three between-group cells: FB-CI, FB-NI, and NFB-CI. These three groups composed the three levels of a single variable, which I called *Information*. The only significant effect from this ANOVA that involved the Information variable was the main effect of Information itself, $F(2, 12) = 5.62$, $p < .02$. The means for the FB-CI, FB-NI, and NFB-CI groups were 17.8%, 15.6%, and 3.8%, respectively. An orthogonal comparison between the FB-CI and the FB-NI groups was not significant, $F(1, 12) = .25$, $p > .5$, showing that these two groups did not differ in degree of success. However, the orthogonal comparison between the feedback groups and the no-feedback group *was* significant, $F(1, 12) = 11.00$, $p < .01$, and showed that these two feedback groups were significantly more successful in alpha control than the group that just received Cognitive instructions. This finding is true for both generate and suppress tasks, since the Information × Task interaction, $F(2, 12) = 2.29$, $p > .14$, was not significant. It is also true for both lights-on and lights-off sessions since the Information × Lights interaction, $F(2, 12) = 3.46$, $p > .05$, did not quite reach significance.

The second special ANOVA consisted of the data from the FB-OI, FB-NI, and NFB-OI groups, which composed the three levels of a single variable (called *Information* again). The main effect of Information, $F(2, 12) = 5.57$, was significant at $p < .02$. The means for the FB-OI, FB-NI and NFB-OI groups were 24.9%, 15.6%, and 12.6%, respectively. The orthogonal comparison between the FB-NI and NFB-OI groups was not significant, $F(1, 12) = .58$, $p > .5$, which showed that persons were equally successful with Oculomotor instructions alone and with feedback alone. However, the comparison between the single information groups and the combined information *was* significant, $F(1, 12) = 10.50$, $p < .01$, and showed that the group that received *both* feedback and Oculomotor instructions was more successful than the groups that received only one or the other. This was also true for both tasks and for both light levels. A significant Information × Trial Number interaction was also obtained, $F(10, 60) = 2.44$, $p < .02$, which showed that the FB-OI group improved across trials to a significantly greater extent than the other two groups.

Subjective Reports

On the postsession questionnaires I asked each research participant in the feedback groups to indicate, by means of 7-point rating scales, the degree of pleasantness, arousal, difficulty, and feelings of success that they experienced during the different types of experimental trials. The data were analyzed by ANOVA 2, with $\alpha = .05$.

Pleasantness

There were no significant effects from this analysis, although one finding should be noted: The main effect of Lights just missed significance, $F(1, 12) = 3.81$, $p < .08$. However, the direction of the effect was that the lights-on sessions were rated *more* pleasant (even though alpha levels were significantly lower when the lights were on).

Arousal Level

Persons in the feedback groups rated themselves to be less aroused during the rest

TABLE 3

Control Strategies Reported by Successful Subjects and Number of Successful Subjects in FB–OI Group

Control strategy	Task: Generate, Lights on	Task: Generate, Lights off	Task: Suppress, Lights on	Task: Suppress, Lights off
Staring at blank wall or letting vision blur	8	3		
Daydreaming, introspection	4	1		
Decreased thinking	1	1		
Multiplication problems	1			
Extreme eye position	1			
Increased awareness of thoughts and feelings	4	2		
Slow eye movement (between focusing)	1			
Outlining, drawing, or imagining visual images or geometric shapes in one's mind				5
Extreme concentration (e.g., on tone)			1	3
Continuous eye movements (rapid series of transient fixations)			6	2
Attending to noises outside the room				2
Focusing vision on objects in the room			8	
Feelings of anger or hate				2
Relaxing (facial muscles)			1	1
Counting or outlining objects in the room			3	
"Trying to get a good feeling for depth perception"			1	
"Blank mind," no thoughts			1	
Number of successful subjects	8	3	8	7

Note. FB-OI = feedback–Oculomotor instruction.

periods than on the generate or suppress trials, which were rated about the same. Means were G = 4.604, R = 3.771, and S = 5.021, where higher ratings refer to higher levels of arousal. This conclusion is supported by a significant main effect of Trial Type, $F(2,\ 24) = 5.76$, $p < .01$, and an orthogonal comparison which showed that G was not significantly different from S, $F(1,\ 24) = 1.24$, $p > .25$, and that G and S combined *were* significantly different from R, $F(1,\ 24) = 10.03$, $p < .005$.

There were differences in arousal level ratings between the generate and suppress trials as a function of the Instruction and Lights variables, as shown by the significant Trial Type × Instruction × Lights interaction, $F(4,\ 24) = 4.53$, $p < .01$. Persons in the CI group rated themselves to be more alert during the suppress periods; persons in the OI group also rated themselves as more alert during the suppress periods when the lights were on, but there is no difference when the lights were off; and persons in the NI group rated themselves as more alert during the generate periods.

Although there was no main effect of lights, persons rated themselves as slightly more aroused during the lights-off sessions (4.458 vs. 4.472).

Difficulty

Persons in the feedback groups felt that the experimental tasks were much easier when the lights were on, regardless of the type of instructions that they had received, as indicated by a significant main effect of Lights, $F(1,\ 12) = 12.37$, $p < .005$ and a nonsignificant Lights × Instructions interaction, $F(2,\ 12) = 1.05$, $p > .38$.

Success

Similarly, persons felt more successful when the lights were on, regardless of instructions, as reflected in the significant main effect of Lights, $F(1,\ 12) = 7.42$, $p < .02$, and the nonsignificant Light × Instruction interaction, $F(2,\ 12) = .10$, $p > .8$.

Subjective Associations and Reports of Control Strategies

On the postsession questionnaires, we asked the individuals in the feedback groups

TABLE 4

Control Strategies Reported by Successful Subjects and Number of Successful Subjects in FB-CI Group

Control strategy	Task			
	Generate		Suppress	
	Lights on	Lights off	Lights on	Lights off
Recalling or imagining pleasant personal experiences ("letting mind go")	6	3		
Relaxation	1	2		
Feelings of floating, flying, lightness, or space	4	1	1	
Visually out of focus	1			
Counting	2			
Sexual fantasies	1			
Frustration	1			
Ignoring tone		1		
Thinking about classes		1		
Detailed visual observation (or focused vision)			4	3
Feelings of anger or fear			1	2
Relaxation			1	
Concentration on tone			1	2
Number of successful subjects	8	5	6	4

Note. FB–CI = feedback–Cognitive instruction.

to describe the strategies they used to increase and decrease the tone volume; and we asked the persons in the no-feedback groups to describe what strategies they used to enter the "receptive" and "active" states. We will now consider the subjective reports of the research participants who showed significant enhancement or suppression of alpha strength, as determined by a one-sample t test with α $(5) = .10$.

Table 3 lists the control strategies of the *successful* persons in the FB-OI group. Only three people showed significant increases above rest levels during the enhance periods with the lights off. All successful individuals in this group used "vision blurring" as a strategy for alpha enhancement. Also popular were "daydreaming" and "increased awareness of thoughts or feelings." Only one person used "extreme eye position" for an enhance strategy. To suppress alpha, all persons used some form of visual focusing or imagination; many also found that moving their eyes around the room and focusing briefly on different objects was a very efficient strategy. Even though the persons in this group were by far the most successful in enhancing alpha levels, they did not report that the enhance trials were particularly pleasant, serene, or tranquil.

Out of a possible 32, there were 26 successful performances by people in the FB-OI group. In comparison, the FB-CI, FB-NI, NFB-OI, and NFB-CI groups had 23, 23, 19, and 10 successful performances, respectively.

As in the FB-OI group, most of the subjective reports of the FB-CI subjects were consistent with what they were led to expect through the instructions they received. As shown in Table 4, the most popular enhance strategies were "recalling pleasant personal experiences" and "feelings of floating, flying, lightness, or space." However, there were seven reports (the most popular strategy) of "focusing vision" in order to suppress alpha. There were only three reports each of "anger or fear" and "concentration" as successful suppress strategies, as the Cognitive instructions had suggested. Evidently, the Cognitive instructions were not effective for alpha suppression, although they did seem to be effective in alpha enhancement when combined with feedback. The relative failure of the Cognitive instructions to aid alpha suppression can be seen by the fact that there were only 10 successful suppress sessions (for the CI group) compared to 15 for the OI group

TABLE 5

CONTROL STRATEGIES REPORTED BY SUCCESSFUL SUBJECTS AND NUMBER OF SUCCESSFUL SUBJECTS IN FB–NI GROUP

	Task			
	Generate		Suppress	
Control strategy	Lights on	Lights off	Lights on	Lights off
Thinking about personal experiences (e.g., sports activities)	4	2		
Sexual fantasies	4	2		
Happy, pleasant feelings		1		1
Feelings of anger, fear, paranoia, frustration, or sadness	2	1		1
Increased awareness of thoughts, lots of thoughts	2	1		
Math equations	1	1		
Thoughts of things subject had to do later	1			
"Blank mind," thinking about nothing			4	5
Detailed visual observation (or focusing the eyes in the dark)			4	1
Changing thoughts (avoiding concentration on one thing)			1	
Deep slow, breathing			1	
Relaxation			2	2
Easy, slow thought			1	
Orienting response, concentration on sounds				2
Concentration on mental objects (images)				2
Number of successful subjects	6	3	7	7

Note. FB–NI = feedback–no instructions.

(out of a possible 16) and 14 for the NI group, which received *no* instructions.

The subjective reports of the FB-NI group, which are tabulated in Table 5, show no *consistent* similarities with the experiences that have been widely associated with high and low alpha states. For instance, these persons did not agree that enhanced alpha states were pleasant. There were seven reports of generate trials being associated with neutral to pleasant states and three reports of the same trial being associated with intense feelings of anger, fear, paranoia, frustration, or sadness. Furthermore, persons who reported generate trials to be unpleasant were no less successful than those who reported these trials to be pleasant. In addition, one individual associated feelings of "contentment" with alpha *suppression*, while no persons associated unpleasant feelings with this task. At least two individuals wrote that they could increase the tone volume by either very pleasant or very negative feelings; evidently the important dimension was the intensity of feeling, or the degree of nonsensory awareness.

Another example is the subjective dimension of relaxation which was more closely related to alpha suppression than to alpha enhancement. No successful participants explicitly mentioned relaxation as an enhancement strategy, although this may have been implicit in some of the reports. In contrast, four persons did explicitly mention relaxation as a suppression strategy.

Again, we found much to our surprise that a majority of the people in the FB-NI group reported a state of "thinking about nothing" or "blank mind" as how they felt during alpha *suppression*.

The persons in the FB-NI group also did not associate the enhanced alpha state with nondirected thought, tranquility, being "high," feelings of lightness or floating, thoughtlessness or "egolessness." In general, we found that the reports of the people who were carefully given no idea as to what subjective changes (if any) to expect were consistent on only one subjective dimension that related to changes in occipital alpha strength, namely, the degree of nonsensory awareness. Specifically, all of the enhance

TABLE 6

Control Strategies Reported by Successful Subjects and Number of Successful Subjects in NFB–OI Group

Control strategies	Task: Generate, Lights on	Task: Generate, Lights off	Task: Suppress, Lights on	Task: Suppress, Lights off
Blur vision, "not looking," unfocusing	5			
Thinking	2			
Daydreaming	1	1		
Relaxing eye muscles	1			
General relaxation	1			
Silent chanting	1			
Detailed visual observation of objects in the room			8	
Detailed visual observation of imaginary objects			1	4
Trying to see in the dark				4
Eye movements (REMs)			2	1
Concentration				2
Number of successful subjects	5	1	8	5

Note. NFB–OI = no feedback–Oculomotor instructions.

alpha reports could be seen as states of increased nonsensory awareness (e.g., paying heed to or absorbed in thoughts and feelings), while most all of the suppress alpha reports were states of heightened sensory awareness (e.g., paying heed to visual objects, sounds, or imagery).

Table 6 lists the control strategies of the successful participants in the NFB-OI group. All reports of the enhance strategies with lights on concern the unfocusing or blurring of vision, as the Oculomotor instructions had suggested. Most of the other enhance strategies concern an increased awareness of thoughts. Evidently, increased awareness of thoughts and feelings often accompanies decreased oculomotor processing. Table 6 also shows that all persons in this group were successful in suppressing alpha with the lights on and five (out of eight) were successful with the lights off; all of these people used a strategy concerned with increased oculomotor functioning.

Finally, the reports of the NFB-CI group (Table 7) show a very interesting phenomenon. Recall that (a) the instructions that this group received made no reference to the visual system, and (b) persons in this group did not receive feedback. However, three of the five persons who were successful in alpha enhancement with the lights on noted that when they were employing the Cognitive instructions (e.g., relaxation, reviewing pleasant personal experiences), they would also discover that at these times they were unfocusing their vision and staring blankly at the wall in front of them. Moreover, on only three sessions (out of 16) were the persons in the NFB-CI group able to suppress alpha with the Cognitive instructions; and one of these persons listed "visual imagination" as her strategy. The relative failure of this group on the suppression task (*no one* successful with the lights on!) lends further support to the idea that the Cognitive instructions were not effective as a suppression strategy.

Discussion

Throughout much of this section I refer to Figure 2, which illustrates in bar-graph form the means for the percent generate success, percent suppress success, and discrimination measures across all of the major independent variables. Note that each discrimination mean equals the sum of the corresponding generate and suppress means. An asterisk marks those means which were not significantly different from zero.

The first question I hoped to answer in this research was: Which control strategy, Cognitive or Oculomotor, is more effective in alpha control, and what is the relation between these two strategies and the relation of each to changes in alpha strength?

The data unequivocally pointed to the Oculomotor strategy as the more successful. Individuals who received the Oculomotor instructions were significantly more successful on all three measures of alpha control than those who received the Cognitive instructions, as can be seen from Figure 2. Even among the three groups that received feedback, persons who received Oculomotor instructions were the most successful. In Figure 2 we can see that the combination of feedback and Oculomotor instructions was by far the most successful combination on all measures of alpha control when the lights were on. We also found that on the generate task, the Oculomotor groups showed significantly greater improvement across trials than the Cognitive groups. Furthermore, the data showed that persons who received feedback and Cognitive instructions did no better than those who received feedback with no task instructions. It is questionable, in fact, as to whether the Cognitive instructions were of any help at all in learning alpha control, especially alpha suppression. No one in the NFB-CI group was successful at alpha suppression with the lights on. The data from the subjective reports suggest that the CI suppress instructions may have made it even harder to learn alpha suppression than no instructions at all, perhaps by misleading the people in the CI groups.

The tight relationship between oculomotor functions and occipital alpha strength showed up in numerous places in the data. Even though their eyes were kept open in all sessions, our research participants had significantly higher absolute alpha strength during the lights-off sessions. We also found, in agreement with other researchers, that alpha control was much harder to learn when the lights were out, most likely due to the lack of visual feedback and the associated destabilization of the eye-positioning control system. Persons did much more poorly with the lights out regardless of whether they had received Cognitive or Oculomotor instructions. Thus, the hypothesis that the absence of light would not greatly affect the performance of people who had been instructed in the Cognitive strategy of alpha control was not supported.

The fact that the absence of light had a greater detrimental effect on the generate task than on the suppress task also would be predicted by the Oculomotor hypothesis. When the lights are out, a ceiling effect occurs on the generate task: Alpha strength is already near maximum levels due to the absence of visual feedback, and one cannot be expected to enhance alpha very much

TABLE 7

Control Strategies Reported by Successful Subjects and Number of Successful Subjects in NFB–CI Group

	Task			
	Generate		Suppress	
Control strategies	Lights on	Lights off	Lights on	Lights off
Relaxation	3			
Blur vision, unfocus, stare blankly at wall	3			
Letting thoughts drift—pleasant personal memories	3	1		
Inward focus	1			
"Blank mind"	1	1		
Passive concentration		1		
Math problems				2
Active concentration on emotions (anxiety)				1
Visual imagination				1
Number of successful subjects	5	2	0	3

Note. NFB–CI = no feedback-Cognitive instructions.

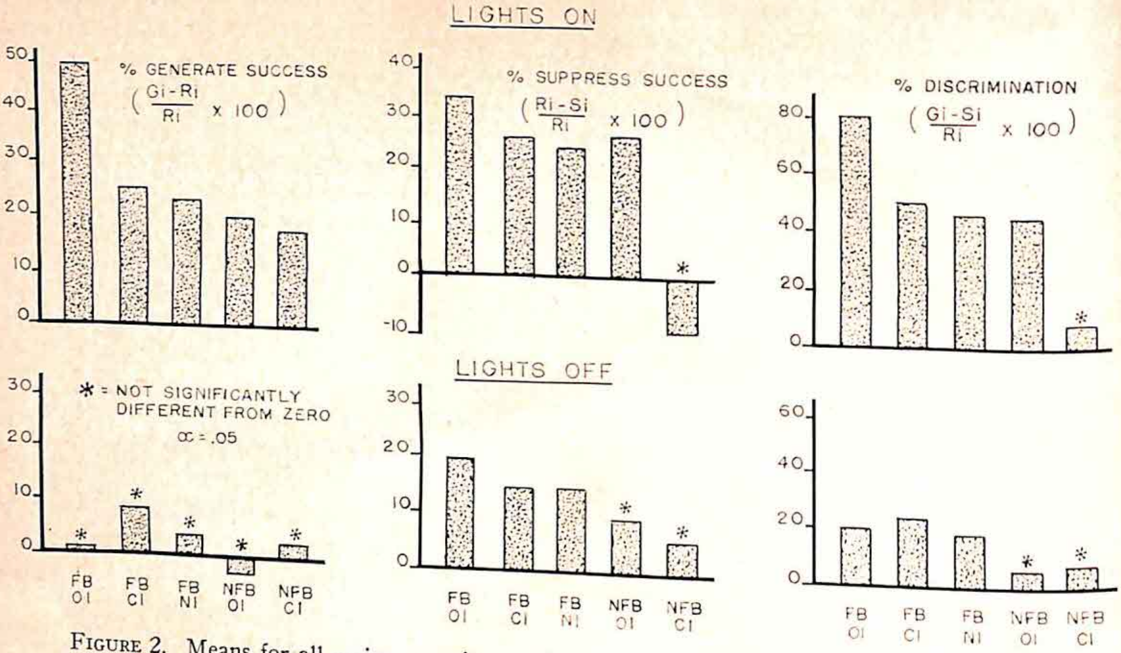

FIGURE 2. Means for all major experimental groups and dependent measures. (G = generate trial, R = rest period, S = suppress trial, i = trial number, FB = feedback, NFB = no feedback, OI = Oculomotor instructions, CI = Cognitive instructions, NI = no instructions.)

beyond this level. The small increases in alpha above resting levels that were occasionally seen during the lights-off sessions were most likely due to the learned inhibition of the residual amounts of oculomotor processing occurring even in complete darkness. On the other hand, it is not as hard for people to *suppress* alpha with the lights off since almost anything they do that affects alpha strength will produce a decrease in alpha since "there is nowhere else to go." However, even alpha suppression is more difficult in the dark since the absence of visual feedback makes it much harder to activate the oculomotor system; it is difficult to "look" at nothing. The fact that the absence of light had a greater detrimental effect on the generate task than on the suppress task regardless of instructions further supports the notion that the self-control of occipital alpha by means of Cognitive strategies that are independent of oculomotor functions is not possible.

The fact that improvement across trials occurred only during the lights-on sessions adds further strength to the proposed connection between occipital alpha and oculomotor functions. In fact, when the lights were off, the degree of success on the suppress task *decreased* across trials. This finding is what would be expected from the Oculomotor hypothesis: When the lights are out, the individual receives no feedback from the environment as to the state of the eye-positioning control system. Therefore, in a 1-hr session with the lights out, it becomes increasingly harder for persons to activate the oculomotor functions and thereby to suppress alpha strength.

We can now ask: What, then, *is* the relationship between Oculomotor and Cognitive strategies of alpha control? In the introduction, I reduced the possible number of such relationships to three. Hypothesis 1 was that there was no consistent relationship between the two; Hypothesis 2 was that the Cognitive strategy was (implicitly) mediated by the Oculomotor strategy; and Hypothesis 3 was that the Oculomotor and Cognitive strategies were independent means of controlling alpha strength. Some findings have already been discussed which allow us to discard Hypothesis 3. The most crucial of such findings is that the main effect of Instructions depended entirely on whether the lights were on or off. When the lights were on, persons who received Oculomotor instructions were much more successful than persons who received Cognitive instructions. However, when the lights were off, alpha was as hard to control with Cognitive strategies as with Oculomotor strategies, suggesting a *common* means of alpha control in both cases, namely the activation and deactivation of oculomotor processes (for alpha suppression and enhancement, respectively). Although a common strategy is implicated, the data quite explicitly demonstrate that (a) persons can and sometimes do learn to control occipital alpha without being aware that they are succeeding by controlling oculomotor processes, but (b) with the lights on, persons are much more successful in alpha control when they have knowledge of the "direct" Oculomotor strategies.

Moreover, the data suggest that, by itself, the Cognitive strategy, as used in this research, does not produce consistent changes in alpha strength. This conclusion is supported by the fact that the discrimination mean for the NFB-CI group is not significantly different from zero (see Figure 2). It is also supported by the subjective reports of the FB-CI group: Although the CIs were consistent with alpha enhancement, the CI suppress instructions were ineffective; in fact, seven out of the 10 persons in this group who were successful in suppressing reported that they did so by an Oculomotor strategy.

Thus, we must conclude that there is no consistent relationship between alpha control and the Cognitive strategy I used in this research (Hypothesis 1). However, an analysis of the subjective reports of the FB-NI subjects suggested that there is a more straightforward "cognitive control strategy" that *is* consistently related to changes in alpha strength (i.e., the degree of nonsensory awareness). Thus, we will accept an altered form of Hypothesis 2. Below we will give further consideration to the nature of this Cognitive strategy.

In short, our answer to the first question is that whenever Cognitive stategies are successful in producing alpha strength changes, it is only due to the fact that they can be efficient mediators of changes in oculomotor processes. The oculomotor system seems always to be involved when an awake individual learns to control his occipital alpha rhythm, even though he may not be aware of this. (However, it is important to note that, although there is a direct relationship between oculomotor activity and occipital alpha strength, it is not a simple *linear* relationship—see Mulholland, 1972, 1974).

The second question I asked was: What are the effects of suggestion or "demand characteristics" on the subjective states reported to occur during the alpha feedback situation? In analyzing the written subjective reports from the experiment, we found that, as expected, the subjective state reports of the FB-CI group were closely related to the instructions the group received, especially for the high alpha state. However, from this result alone we do not know whether the subjective experiences of this group were intrinsic correlates of the alpha strength changes or of the person's expectations or of both. This question was answered by looking at the subjective reports of the group that received feedback but was given no expectations as to what experiences may or may not occur. Although the persons in this group did as well in alpha control as the FB-CI group, their reports showed no *consistent* similarities with the experiences that have been widely associated with high and low alpha states. Evidently, when people are not instructed as to what kinds of subjective states they should expect to experience during alpha training (i.e., when demand characteristics are minimized) they are not likely to describe the alpha state as unusual or particularly beneficial or enjoyable.

Moreover, we were surprised to find that a majority of the individuals in the FB-NI group reported a state of "thinking about nothing" or "blank mind" as how they felt during alpha *suppression*. This was surprising because reports of "blank mind" are often associated with meditative states, which are also reported to be states of *enhanced* alpha. However, the subjective reports gathered from the present experiment suggest that when a biofeedback trainee without any meditation experience reports a state of "blank mind," he is probably saying that he was paying close attention to the sensory world and had reduced (nonsensory) awareness of thoughts and feelings. On the other hand, when an advanced meditator reports a state of "blank mind," he usually means that he was neither observing the sensory world nor heeding thoughts and feelings. Therefore, during states of "blank mind," the biofeedback participant is engaging in a high level of oculomotor processing (alpha suppression), while the meditator has greatly reduced oculomotor processing (alpha enhancement).

A few additional aspects of the data from the postsession questionnaires are relevant here. Persons rated the generate trials of the lights-off sessions to be less pleasant than the generate trials of the lights-on sessions even though the alpha levels of the former were significantly higher than those of the latter. In addition, I found no striking relationships between arousal level and alpha. For example, even though participants' alpha levels were significantly higher during the lights-off sessions, they rated themselves as slightly more aroused during these sessions (although not significantly). Finally, although persons in the FB-OI group were by far the most successful in enhancing alpha levels, they did not report that the generate trials were particularly pleasant or serene.

The reports of the persons in the FB-NI group were consistent on only one subjective dimension that related to changes in occipital alpha strength—namely, the degree of nonsensory awareness. All of the enhance alpha reports were of states of increased (nonsensory) awareness of thoughts and feelings, while nearly all of the suppress alpha reports were of states of decreased nonsensory awareness or heightened awareness of the sensory world.

This findings fits in very well with the Oculomotor hypothesis: Since vision is the dominant sensory modality in man, it is reasonable that when oculomotor processes are reduced that there is also reduced sensory awareness in general and greater awareness of thoughts and feelings (i.e., nonsensory awareness). Likewise it makes good sense that when the oculomotor system is active, persons are more aware of the sensory world and less aware of thoughts and feelings, often leading, in fact, to reports of "blank mind." Several serendipitous findings from the subjective reports of our research participants strongly support this conclusion. For example, many persons in both of the OI groups reported that in order to follow the instructions for the generate trials (which were to blur their vision and to be visually inattentive), they engaged in various strategies that amounted to daydreaming or becoming increasingly aware of thoughts and feelings. In addition, a few persons in these groups reported a feeling of "blank mind" while carrying out the Oculomotor suppress strategy of increased *sensory* (visual) awareness. Another example is one individual in the NFB-CI group who was particularly successful in generating alpha levels above resting levels (most persons in this group had failed). On his subjective report questionnaire he wrote that he entered this state by "a relaxation and a focus primarily inward" (consistent with the CIs) and that he noticed, incidentally, that when he was successful at this "inward focusing" his *vision* would be blurred and *out* of focus. Recall that as a member of the NFB-CI group, this individual had received no feedback signal and he was given no instructions that made any reference to the Oculomotor strategies. Two other persons in this group also noted that when they were employing the Cognitive strategies they would discover that they were also, at these times, unfocusing their vision and staring blankly at the wall in front of them. In another case, a person in the FB-NI group reported that to suppress the alpha tone, she focused visually on her dress pattern while trying to make her mind blank and "project thoughts externally." These and other unsolicited reports illuminate the frequent close coordination between the activation of the visual system and the degree of sensory awareness.

The connection between occipital alpha strength and the degree of sensory awareness also allows us to explain a puzzling finding which is shown on the middle graph of Figure 2. This is the fact that on the suppress task, the NFB-CI group actually showed *increases* in alpha strength over rest period levels. This finding can be explained in the following manner: The instructions to our research participants for the rest periods were to do anything they like except to practice the control strategies. However, *both* the generate and suppress strategies of the Cognitive instructions described subjective states that involved an awareness of thoughts and feelings, although they differed in the type, intensity, and affective valence of thought and feeling. Thus, during the rest periods, many of the persons in this condition entered states of increased sensory awareness in order to refrain from practicing either their generate or suppress strategies. This resulted in lower alpha strength during the rest periods compared with the suppress periods. This reasoning also explains why this group did as well as it did on the generate task.

The third question I asked was: What was the role of feedback *itself* in the learning of alpha control? Jackson Beatty (1972) has reported that task information is a factor that is as effective in alpha control as physiological feedback. However, I found that the individuals in my experiment who received feedback were significantly more successful on all measures of alpha control than those people who received task instructions and no feedback, especially with the lights on (see Figure 2). However, there are two considerations that must be pointed out in order to adequately interpret this finding. The first concerns two procedural differences between Beatty's study and mine, and the second concerns the confounding between the superiority of feedback and the inferiority of the Cognitive instructions alone.

There are many procedural differences between Beatty's study and the present one, but two are likely to be of greatest significance. The first concerns the dependent measures. I used an integrated amplitude measure of alpha *strength*, which is sensitive to both the amplitude and abundance of alpha, while Beatty used a percent time index of alpha *density* which is only sensitive to alpha abundance. In addition, Beatty did not include baseline periods, which may have led to relatively insensitive measurements of alpha control. Thus Beatty may not have found differences between feedback and no-feedback conditions due to a relatively insensitive methodology. However, the second consideration concerning the differences between our studies may very well be more important. This has to do with the differences in our Cognitive instructions.

In the introduction to this report I quoted Beatty's task instructions in full. Although they are similar to mine and to those reported in the literature, he did emphasize the dimension of sensory/nonsensory attention more so than I did. Included in his instructions were (a) beta is associated with the orienting response to novel stimuli and to sensory attentiveness, and (b) alpha is associated with increased inner awareness. Perhaps enough of his information-only subjects followed these aspects of the instructions in order to produce significant differences in percent time alpha.

The two special ANOVAs I employed are relevant here. The group that received both feedback and CIs did not do any better than the group that received just feedback, while both these groups did better than the group that received CIs alone. Thus, persons cannot learn to effectively control occipital alpha by merely being informed as to the Cognitive strategies as used in this research. However, we also found that the FB-NI group did no better than the NFB-OI group, which suggests that instruction in the Oculomotor strategy alone *is* as effective in alpha control as physiological feedback. We can see Beatty's findings as consistent with this aspect of mine, if we keep in mind the close correlation between the degree of nonsensory awareness and the degree of oculomotor activation. However, Beatty also reported that his research participants who received both feedback and task information did no better than persons who just received one or the other. As concerns the Oculomotor instructions, my findings are not consistent with this: I found that the FB-OI group did do significantly better than either the FB-NI or the NFB-OI group. This suggests that although feedback and Oculomotor instructions have similar effects on the learning of alpha control, the combination is significantly better than either alone.

In sum, it appears that to a large degree, the differences between Beatty's findings and mine are due to the fact that his task instructions are somewhere in between my Cognitive and Oculomotor instructions in effectiveness. Thus, (a) Oculomotor instructions improve alpha control beyond that obtainable with feedback alone, (b) Beatty's task instructions do not produce such improvement, but they are *as* effective as feedback alone, and (c) my Cognitive instructions are not even as effective as feedback alone.

There is another difference between Beatty's findings and mine. He reported that his information-only subjects improved across trials in a similar manner to his feedback subjects. However, I did not find this. My feedback subjects showed large increases across trials, while my no-feedback subjects showed almost no improvement.

Feedback significantly improves the self-control of occipital alpha strength beyond that which can be achieved by instructions alone since oculomotor responses can be very subtle and automatic, often going unnoticed, which, of course, makes them very difficult to control to high degrees without feedback. However, with the addition of feedback, the subtle changes in oculomotor processing can be effectively monitored, allowing us to determine how to bring them under control with relative ease. The usefulness of feedback is greatest in achieving very high degrees of oculomotor relaxation. For example, when the lights are off, baseline levels are near maximum since the eye-positioning control system is relatively unstable. At these high levels of oculomotor relaxation, further relaxation is difficult to achieve by simply willing it. This is where the availability of a feedback signal is most useful, as demonstrated by the fact that 46% of the persons in the FB group were able to enhance alpha with the lights off, while only 19% of the NFB subjects were able to do so.

On other essential issues, Beatty and I are in agreement. In his 1972 paper, he concluded that

> the fact that very simple and very general instructions can effectively induce alpha wave activity argues against the popular notion that the alpha state is quite unique, attainable only by meditation or EEG contingent reinforcement. (p. 153)

In sum, evidence from the present research has led to the conclusions that (a) in the feedback augmentation of occipital alpha, enhancement and suppression of this rhythm is always mediated by learned control of oculomotor processes, although sometimes persons are not aware that they are employing this strategy, especially in the case of alpha enhancement, (b) the earlier reported association between the alpha experience and occipital alpha strength can be explained in terms of the instructions or the "demand characteristics" of the experimental procedures, (c) there *are* reliable correlations and understandable associations between occipital alpha strength and the degree of nonsensory awareness, and (d) the most effective method of controlling the occipital alpha rhythm is by means of feedback together with simple Oculomotor instructions.

Occipital Alpha and States of Consciousness

Let us now take a closer look at the connection between occipital alpha and states of consciousness, and at the possible utility of alpha feedback as a meditation technique or as a therapeutic device, two related issues which are of the greatest importance to those interested in brain wave feedback, and which therefore deserve further comment.

What we have seen from the research described here is that there is no *intrinsic* connection between a neurological state of enhanced occipital alpha and a meditative state of consciousness or the state of conciousness referred to in the literature as the "alpha state."

The picture we have now is that to enhance occipital alpha strength a biofeedback trainee will learn to relax his oculomotor system. The easiest way to accomplish this is to cease paying heed to the visual modality (although one *could* learn to pay close attention to color, shape, etc., while still enhancing alpha). But since vision is the dominant sensory modality, our trainee will likely discover that an easy way to become visually inactive is to not pay heed to *any* sensory modality (although one *could* learn to attend to, e.g., sounds without being visually active). Moreover, our trainee will soon discover that the easiest way to maintain a state of sensory inactivity without falling asleep is to simply pay close attention to—or to become absorbed in—his thoughts (although one *could* learn to stay awake while paying heed to neither the sensory world *nor* one's thoughts—this is often the "goal" in meditative practices, although anyone who has ever tried meditating will tell you that stopping thought for any length of time while sitting still in a meditative posture is far from simple). In essence, we can see that it makes sense that the easiest strategy for enhancing occipital alpha strength would be to become absorbed in one's thoughts or feelings, since this is a simple way to maintain a state of oculomotor relaxation.

On the other hand, since the "alpha state" *is* one instance of a nonsensory state of consciousness, the attainment of an "alpha state" can be an effective strategy for alpha enhancement, but only for those persons who already know how to enter such a state of consciousness. The problem arises when we talk about alpha feedback as *inducing* an "alpha state." This implies that the "alpha state" *always* accompanies enhanced alpha, and that, therefore, if we can train people to enhance alpha strength, then they will succeed in self-inducing an "alpha state." However, it should be clear by now that this is not the case. Entering an "alpha state" is only one way of enhancing occipital alpha; any other mood state—regardless of the contents, affective valence, or even presence of thoughts and feelings—would work equally well as long as the person is also not looking. Therefore, we have no guarantee—nor do we have any grounds on which to expect—that a person in alpha feedback training will enter an "alpha state" by virtue of learning to enhance alpha strength. As we saw earlier, there *is* a reliable correlation between enhanced alpha and a state of nonsensory awareness, but how a person reports this state of nonsensory awareness is in no way a function of occipital alpha strength. Thus, simply conditioning enhanced alpha strength is not a way to induce an "alpha state" in the same sense that simply conditioning a certain kind of arm strength is not a way of transforming someone into a star quarterback (although one will most likely find that star quarterbacks have strong throwing arms).

An illustration of a logical mistake concerning the relationship between occipital alpha and states of consciousness is an hypothesis offered by Nideffer (1973). He suggested that "within an individual, as affective arousal (emotional involvement) increases, alpha will decrease. That is, alpha should be produced so long as stimuli are not being reacted to in an ego-involved way" (p. 176). Evidently Nideffer made this hypothesis based on the connection between meditation and calm, relaxed, "ego-free" states, and the reported connection between meditation and alpha. However, just because enhanced alpha occurs during these two different situations, there is no *logical* grounds for equating these situations on any *other* dimensions, such as egolessness. In fact, the results of my research have lent strong support to the opposite conclusion—namely that alpha is often produced when persons are in ego-*involved* states, in the sense of being absorbed in personal thoughts and feelings. However, no intrinsic relation between ego involvement and alpha is implied here. The only direct connection involved is between occipital alpha and oculomotor processes. In addition, we have found, in the alpha feedback situation, a statistically reliable connection between a reduction in oculomotor processing and enhanced nonsensory awareness. The latter happens to be often characterized by increased ego involvement in meditation-naive persons, and by decreased ego involvement in meditation. In short, there appears to be no reliable connection between occipital alpha strength and any specific state of consciousness or any specific type of nonsensory awareness. Therefore, when operantly conditioning enhanced occipital alpha strength (reduced oculomotor processing) we would not expect (or want to claim) that we are *thereby* also inducing an altered state of consciousness.

It should be noted here that an increase in occipital alpha strength is only one of the several changes in EEG that occur during meditation. The above critique does not imply that none of these EEG changes (e.g., slowed frequency, alpha enhancement from other cortical locations) uniquely accompany meditation, although this could be the case since no well-controlled research has provided evidence to the contrary. However, a statistically significant correlation between a physiological state of affairs and, for example, a verbal report of having experienced an altered state of consciousness is never grounds for identifying the altered state of consciousness with the physiology or for inferring any causal connection between the two. Moreover, although such a correlation, if found, may help us to *study* the state of consciousness in question, it does not in itself further our *understanding* of the state of consciousness. (See Plotkin and Schwartz, Note 5, for a formal explication of the concepts of altered states of consciousness and hypnosis and their relationship to physiology.)

In order to make sense out of the earlier findings associating alpha activity with the alpha experience, Walsh (1974) conducted a noteworthy experiment investigating the interactive effects of alpha feedback and instructional set on subjective state. In his study, alpha enhancement and alpha suppression feedback were each paired with alpha experience and neutral instructional sets. He found the typical alpha experience to be reported only when people were given *both* the alpha experience set and alpha enhancement feedback. Either alone was not sufficient. Walsh interpreted his results in the following manner:

> The so-called alpha experience may be associated directly with the occurrence of alpha rhythm activity, as is commonly believed, but the subjective changes involved may be so subtle as to be easily blocked by situational factors. As a result, these changes may only become apparent when the situation provides appropriate preparation for the experience, including some concepts to use in describing it. (p. 433)

However, I will argue that this conclusion has not been shown to be empirically warranted by Walsh's data. First, let us look at three possible positions concerning the relationship between alpha activity and the alpha experience:

First is Walsh's position that the alpha experience *is* associated "directly" with the alpha rhythm, but that this association is often masked by situational factors.

Second is the position which holds that an alpha experience is reported only when the person is given the appropriate instructional information, regardless of the presence or absence of alpha activity (Beatty, 1972).

The third position, which I am propounding in this paper, holds that (*a*) there is no direct association between the alpha experience and alpha activity; there is no such association to be blocked by situational factors; (*b*) there very often *is* a reliable correlation between nonsensory awareness and alpha activity; situational factors do not block this correlation; (*c*) the alpha experience is only one instance of nonsensory awareness; (*d*) an appropriate instructional set is not necessary for the occurrence of an alpha experience, although such an experience in the context of feedback training is very much more likely if a person *has* been led to expect it; and (*e*) alpha feedback per se is neither necessary for nor especially facilitative of the achievement of the alpha experience.

First we will discuss Points *a*, *b*, and *c* above. Although there is good reason to expect that an alpha experience, since it is a nonsensory state, will not be reported if a person was not exhibiting alpha activity at the time, this does not imply that the alpha experience is *directly* associated with alpha activity or that "situational factors" can block this association. What Walsh's study showed was that if a person is being visually active (during alpha *suppression* feedback), he will probably not report *any* kind of nonsensory state, including the "alpha state." Thus Walsh has not shown that the alpha experience is directly associated with alpha activity, but rather that the *absence* of alpha activity is often associated with a state of *sensory* awareness which most often excludes the possibility of an alpha experience *as well as all other nonsensory experience* such as increased awareness of, for example, paranoid thoughts. Moreover, situational factors do not block the association between enhanced alpha and nonsensory awareness, as illustrated by the fact that nearly every report from the persons in my research who were successful at enhancing alpha were reports of states of nonsensory awareness.

Point *d* above is opposed to both Walsh's and Beatty's positions. I hold that instructional set is not a *necessary* prerequisite for the alpha experience. A person is simply much more likely to describe his state of nonsensory awareness in terms of an alpha experience if he has been led to expect this kind of experience. However, certain persons will occasionally report an alpha experience without having received an instructional set when they merely enter a state of nonsensory awareness during the special circumstances (see below) of a biofeedback experiment, as occasionally occurred in the FB-NI group of my experiment.

Point *e* above deserves careful consideration. A finding from my research, as well as that of Walsh (1974) and Beatty (1972), is that persons who reported the alpha experience were, at those times, exhibiting alpha strength levels that were usually lower or occasionally just slightly higher than these same persons' alpha strength levels when they simply closed their eyes or sat in a dark room with their eyes open. Thus, alpha feedback per se is neither necessary for nor especially facilitative of the occurrence of an alpha experience since, in order to have an alpha experience, it is not necessary to achieve alpha strength levels above those obtainable by simply closing one's eyes or by not looking. The only prerequisite for an alpha experience is that a person be in a state of nonsensory awareness, and alpha feedback is in no way necessary for the achievement of such a state (e.g., daydreamers do this all the time "on their own").

Although alpha feedback per se is neither necessary for nor especially facilitative of the occurrence of an alpha experience, there are good reasons to believe that the alpha feedback *situation* can facilitate the occurrence of such an experience. Consider some of the other significant factors besides explicit suggestion and feedback that are commonly involved in the alpha feedback training situation: (a) the research participant must be constantly monitoring a feedback signal; thus there is often sustained attention; (b) the physical setting—prolonged sitting in a soundproof, darkened room—often amounts to moderate sensory deprivation (see below); and (c) the participant finds himself in a strange laboratory environment containing unfamiliar electronic gadgetry with all the connotations and associations that such an environment may be expected to conjure up. These factors and others may be expected to have significant effects on how a person reports his experience during an alpha feedback experiment.

When we consider the effects of these three factors along with the effects of the individual's personal characteristics, his expectations, and the degree of experienced success, the suggestion that there is no direct association between the alpha experience and alpha activity per se becomes much less extraordinary. Yet, at the same time, we can understand why it once appeared as if there were such a direct association to researchers who were not being sufficiently critical, who were absorbed by the idea of a direct and simple relationship between states of consciousness and neurophysiology, and who were not considering the other possible explanations.

Another hypothesis related to the understanding of the alpha experience is that which is offered by Peper (1971). Several research reports (Chatrian, Magnus, Petersen, & Lazarte, 1959; Jasper & Penfield, 1949; Klass & Bickford, 1957; Kreitman & Shaw, 1965) have suggested that the occurrences of alpha blockage at cortical locations other than the occipital lobe are also due to efferent motor commands initiated from the cortical location in question. Thus, in general, alpha enhancement would be expected to accompany a state of sensory limitation (cessation of efferent motor commands). Moreover, due to the dominance of vision in man, alpha enhancement in the occipital lobe alone (i.e., cessation of oculomotor activity) would be expected to have a generalized sensory limitation effect. In this connection, Peper hypothesized that "the subjective changes of consciousness reported during the high alpha states are due to the absence of efferent motor commands" (p. 230), which leads to a state of sensory limitation. Such a state of sensory limitation, which may also be expected to be characterized by increased *non*sensory activity (e.g., introspection, daydreaming), is probably a mild instance of the sensory deprivation experience (see Lilly, 1956; Zubek, 1969). As Lilly reported, such sensory deprivation experiences may be reported as extremely fascinating and enjoyable or as frightening and psychotomimetic, depending on the person and on "set and setting." We can understand alpha enhancement to accompany a similar state, somewhat milder than sensory deprivation, which also may be reported as either pleasant or unpleasant, or as simply boring.

As for the clinical applications of occipital alpha feedback, I have seen no well-controlled research to date that gives us any reason to believe that alpha feedback, as a meditation technique or as a therapeutic device, is as effective as any of the traditional meditation exercises, or as progressive relaxation (Jacobson, 1938), autogenic training (Luthe, 1969), systematic desensitization (Wolpe, 1958), hypnosis, or self-hypnosis. This is not to say that the alpha feedback *situation* is never effective—there are simply no grounds on which to assert that alpha feedback per se is a significant aspect of those situations in which alpha feedback has been employed in a therapeutic context. The possible benefits of alpha feedback training will depend upon, among other factors, how it is being used (including what it is being used in conjunction with), who is using it, and for what purpose it is being used.

Concerning meditation and alpha, consider the following two points.

1. Since some aspects of the behaviors engaged in during alpha feedback training are similar to those of meditation practice—such as constant monitoring, "passive volition," and sensory limitation—there may be reasons why alpha feedback could serve as a potential meditation technique under certain circumstances. However, it is most important to understand that the *significance* of meditation concerns the *practice* of certain *behaviors* (e.g., nonattachment, awareness of breathing, passive volition) and not the *achievement* of certain *goals* (e.g., the attainment of any particular physiological state of affairs). The physiology of persons who are meditating is totally incidental to the significance, for meditators, of meditation. Thus if alpha feedback could serve as a meditation technique, it would only be by virtue of the behaviors engaged in under the special circumstances of feedback training (e.g., constant monitoring of the feedback signal) rather than by virtue of any similarity between enhanced alpha activity and the physiology of persons who are meditating. Moreover, we will briefly discuss below an important reason why, in any case, alpha feedback would not be recommended as a meditation technique.

2. It makes sense, as Kamiya (1969) and others have reported, that meditators are good at alpha enhancement, simply because they have had much practice and experience in entering a nonsensory state which is incidentally characterized by decreased oculomotor processing. However, the fact that

oculomotor relaxation accompanies meditation in no way implies that a meditative state can be attained by relaxing the oculomotor system.

In conclusion, we should not be unduly disappointed that there is no direct association between enhanced alpha and the alpha experience. The chain of research on alpha feedback, from Kamiya's (Note 1) first paper to the present, has been valuable in showing us that, although we once thought that a box of amplifiers and filters had made it possible to induce a desirable state of consciousness more rapidly and effectively than ever before, in fact we were really always doing it "on our own." We simply discovered once again that often people only need a certain degree of faith in their natural powers and abilities, along with an appropriate setting and simple instructions, in order to accomplish what they feel is normally beyond their potential.

It is in this connection that alpha feedback would not be recommended as a meditation technique since it would tend to create the self-limiting illusion that our altered states of consciousness are a causal function of our physiology—a physiology that requires an electronic aid for us to alter. We have already gone through this unfortunate learning experience with psychedelic drugs, which reinforced the restricting notion that certain chemicals are a prerequisite for attaining certain desirable states of consciousness (Weil, 1972). We need not repeat this "conditioning" with biofeedback. The power to enter altered states of consciousness is a *natural* ability that we all can potentially tap; learning how to do this without *external* devices such as electronics and drugs will serve to expand our behavior potential in the widest range of circumstances (Plotkin & Schwartz, Note 5).

Biofeedback does represent an invaluable advancement in our capacity to introduce ourselves to whole new realms of bodily self-control; the present research should in no way be taken as a disparagement of this new tool. In fact, it is *because* of its great potential that we should be most careful not to make premature claims for biofeedback, which will only result in a growing suspicion of this young field. Finally, let us be careful to understand that biofeedback equipment never does anything *to* us; rather, *we use* biofeedback devices to actively help ourselves explore and improve our own *natural* abilities.

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Some Comments on Plotkin's Self-Regulation of Electroencephalographic Alpha

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We are pleased at the opportunity to respond to Plotkin's (1976) article. Like so many of the follow-up studies on alpha feedback, this one raises questions and offers answers on issues that have been important since the first studies. In reflecting on Plotkin's article, two objectives stand out with particular prominence. The first of these concerns the attempt to explain alpha activity (with and without feedback) as primarily due to oculomotor effects, and the second involves the effort to reinterpret previous experiential descriptions of the high alpha state and to substitute a new description based on the data of this study. These two objectives shall be considered in some detail, because we believe the objectives were not satisfactorily achieved.

The Oculomotor Hypothesis

It should be noted immediately that the single channel of electroencephalogram (EEG) data was derived from a bipolar frontal to occipital placement. It is therefore misleading to refer, as in the article's title, to the occipital alpha rhythm when it could be called the frontal alpha rhythm by the same logic. The use of a frontal site (near the eyes) considerably increases the oculomotor disruption of any recorded alpha activity over and above that which would be seen in an occipital-to-temporal, an occipital-to-occipital, or an occipital-to-earlobe electrode scheme. This disruption is especially strong with vertical eye movements, which can cause an apparent loss of the EEG for seconds at a time. This is due to the fact that the electroocular potential, traceable to the rotation of the corneo-retinal dipole, can be much larger than the EEG, depending on the size and direction of the eye movement. The larger movements can easily saturate the EEG amplifier momentarily, causing no apparent EEG for the duration of the saturation. There are methods available for eliminating this artifact (Girton & Kamiya, 1973), but they require additional circuitry.

Since eye movement is one of many factors statistically associated with reduction in EEG alpha, the use of a frontal electrode site would result in an apparent accentuation of this association in the feedback experience of the trainee. Unfortunately, the magnitude of the resulting errors in assessing alpha cannot be evaluated, especially since it seems that Plotkin did not make graphic records. (He mentions only the Bio-feedback Systems, Inc., equipment in the apparatus section.) All we can do is caution against the use of frontal reference electrodes unless the eye movements can be eliminated, minimized (as verified by recordings), or otherwise accounted for. We must keep the frontal site in mind when considering Plotkin's conclusion that "feedback-augmented enhancement and suppression of occipital alpha strength is always mediated by learned control of oculomotor processes" (p. 66). Indeed, we must contend that this conclusion is stated far too strongly for a study which did *not* monitor or record a single measure of oculomotor activity.

Plotkin arrives at his oculomotor conclusion after formulating and considering three alternatives for possible relationships of oculomotor and cognitive strategies to alpha control. We do not believe that his three alternatives are the only reasonable

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possibilities. At least one additional alternative should have been considered, namely, that the cognitive and oculomotor strategies are differentially effective, depending on whether the control is in the direction of reduced or enhanced alpha, relative to resting baseline. A question that troubles us about Plotkin's alternatives is raised most clearly in his second alternative, that cognitive processes may control alpha "only to the extent that they successfully *mediate* changes in *oculomotor functions*" (p. 73; italics added). We assume that by *mediate* he means *cause* or *produce*, and by *functions* he means some physiological processes of the oculomotor system. This is a hypothesis about how training of alpha control is made possible physiologically, and as such does not imply any necessary behavioral or psychological strategies. Assume for the moment that alpha activity is completely determined by the oculomotor system, and specifically in such a manner that with activation of the oculomotor system, as in visual scanning of the environment, there would be alpha suppression, while with maximal quiescence of this system, there would be maximal alpha. The question of what would cause maximal quiescence would be open. It might be drowsiness, it might be some specialized state of consciousness, as well as what many think it could only be—a deliberate voluntary relaxing of the eye muscles. The fact that knowledge of alpha's physiology would not necessarily help us understand it psychologically raises doubts in our minds as psychologists as to why the physiological question is raised. We hope it is not a belief that if it is physiological, no cognitive processes could be involved.

Considering that Plotkin did not obtain measures of oculomotor activity as a direct check of his hypothesis, we are led to assume that what he really meant was that oculomotor strategies (as established by experimental instructions or as inferred from verbal reports) might be involved in executing cognitive strategies (established or inferred in the same way). So long as there is understanding that a physiological hypothesis of alpha control was really only tested at the behavioral level, we can proceed. However we believe that Plotkin did not stay at the behavioral level in the discussion of his results. Plotkin's zeal in embracing the oculomotor hypothesis can be traced to his citations of work by Mulholland (1968), Mulholland and Peper (1971), and Paskewitz and Orne (1973). However we should note that normal alpha activity has been recorded in eyeless subjects missing one or both eyes, even when there was complete exenteration (removal of eyeball, extraocular muscles, and periosteum). This finding by Chapman, Cavonius, and Ernest (1971) supported their view that alpha activity is "not directly dependent on the corneoretinal potential of the eyeball, tremor of the extraocular muscles, eye position, accommodation, or eyelid flutter" (p. 1161). While Chapman et al. did not employ feedback training (which would be an interesting study to do), they did show that alpha in eyeless subjects showed the normal decreases during "hard" mental tasks. In view of these findings, it would seem incautious to suggest that feedback control of alpha is "always mediated by learned control of oculomotor processes, although sometimes persons are not aware they are employing this strategy" (p. 66).

There are at least two phases to alpha feedback training as has been shown by Hardt (1974, Note 1). The first phase lasts about 2 hr and is primarily devoted to adaptation and habituation (Hardt & Kamiya, Note 2), that is, eliminating those processes which block alpha. The second phase begins after about 140 min of training and may represent positive achievement at alpha enhancement. This process may be different from elimination of blocking effects and the shape of the acquisition curve is different from that seen during the first 2 hr. There are even hints of a third phase beginning after about 6 hr of total training. Plotkin's total feedback training time, including both generate and suppress, was 72 min, so his trainess never really got into the second phase which is alpha enhancement; they were still working on elimination of alpha-suppressing effects. Although Plotkin describes the time for learning as "ample," we see this is not the case. Furthermore, to anticipate the second objective, we can note that since some of the subjective experiences usually referred to as the "alpha experience" do not begin to occur until *after* the 2-hr adaptation and habituation phase, the data do not warrant a reinterpretation of the alpha experience. Indeed the findings of Walsh (1974) that the alpha experience depends upon both alpha activity and expectation of an alpha experience (set and setting) may be valid *only* during the 2-hr adaptation period, because Walsh also provided less than 2 hr of training and his trainees did *not* even significantly enhance alpha activity.

It has been our experience that giving performance scores (digital) to the trainees at the end of each trial or epoch helps in sustaining motivation and avoiding drowsiness or getting "lost." On the basis of the feedback tone alone, trainees are often unable to form impressions as to whether they have done better or worse on a given trial. Thus without such a digital score they can become discouraged or focused on an unduly narrow subjective time window and be unable to assess the qualitative nature of the task. At the very early part of alpha training, trainees sometimes find the scores distracting, but they soon learn to rely on them for indexing their progress.

In a study which had as one of its objectives the assessment of the subjective qualities of enhanced alpha, the requirement of training with eyes open in a lighted room for half of the total training time is surprising. It is rather like asking persons to experience a state which they are prevented from experiencing. By requiring his trainees to keep their eyes open, Plotkin is guaranteeing oculomotor disruptions of alpha activity. To be sure, these disruptions are less in the dark than when the lights are on, but it is misleading to consider the gradual recovery from the light-induced disruption as "alpha learning." In fact Plotkin's success scores obscure the "recovery" aspect of the training by eliminating absolute magnitudes of alpha activity. We commend this study for its use of equipment which provided feedback and quantification as a continuous function of alpha amplitude (rather than dichotomous feedback and percent time scores); however Plotkin apparently does not recognize that eyes-open plus light produces severe depression of alpha activity, which the short period of feedback training merely begins to undo. Having apparently decided in advance of the data that oculomotor activity is the chief determinant of alpha activity, he misses an alternative interpretation of his percent discrimination scores in his Figure 2. With lights on, feedback plus oculomotor instructions produced the greatest "control" by trainees. But with lights off, feedback plus cognitive instructions was best, even though we know there must have been some oculomotor disruption due to open eyes and "trying to see in the dark." We suspect that if his trainees had used closed eyes in the dark, the superiority of cognitive over oculomotor instructions would have been even greater. In fact, the direction of his effects in the no-feedback (just instructions) groups also supports this view.

Plotkin reveals his belief that people cannot increase alpha above an optimal resting baseline by such comments as: "With the lights off . . . almost anything they do that affects alpha strength will produce a decrease in alpha since 'there is nowhere else to go'" (p. 88). We see that he designed the feedback training in such a way as to minimize the chances of seeing what he did not expect: (a) By eyes-open training and the use of light, alpha levels are depressed below eyes-closed resting levels. (b) By giving only 36 min of enhancement feedback it is virtually guaranteed that trainees would not even recover to eyes-closed resting levels. (c) By frequently interrupting the trainees (every 3 min) to change the task, it is guaranteed that the subtle psychoneural state of high alpha will not have much time to develop and will probably not be seen. (d) The technique of scoring success by comparing each trial against its closest baseline measure might be helpful in locating short-term differences in state as effected by the instructions. This advantage is offset, however, not only by its training disadvantages as mentioned earlier, but by obscuring any carry-over effects from all the previous training. One cannot assume such effects would be absent just because subjects were instructed not to do anything. Such a procedure implies the assumption that people have sufficient control over alpha to "not do it" during the baselines. This assumption of control before the proof of control led to a hasty dismissal of Kamiya's (1969) interpretation of rising baselines (in a similar situation) as "not entirely convincing." No reasons for the alleged insufficiency of Kamiya's interpretation were given, and unreasonable demands were placed on the baseline situation. For example, it may be unwise to insist that the baseline *not* change as a result of the training. If, for example, one wanted to know how breathing rates changed as a result of exercise, one could take a prior resting baseline measure and another measure during the exercise. But an insistence that resting baselines taken after some of the exercise show the same rates as the initial resting baseline would be misguided if the exercise consisted of climbing up or down a 10,000-ft. mountain. The air pressure and available oxygen change with altitude, and so do the "resting" breathing rates. It may be that alpha feedback done with proper methodology can carry the trainee to new "alpha altitudes" from which alpha activity might not quickly recede, even if a baseline period was proclaimed. This possibility is overlooked by Plotkin's design and his discussion.

Even with a design using participants as their own control, one *can* assess what happens as a consequence of the mere passage of time, by having sufficiently long and enough pretraining baseline sessions. The ultimate question is whether the total training has effects that go beyond whatever trends there may be before training. Unfortunately the study does not make possible such before/after comparisons.

Summary of Oculomotor Results. We refer the reader to Figure 2 in Plotkin's article, which graphically summarizes most of the important results. It is clear that feedback is effective in producing changes, at least in light, for all instruction groups, and that oculomotor instructions are superior to cognitive instructions, again in the light. After reviewing these and other findings, Plotkin decided that a modified version of his second hypothesis best fits the evidence, to wit: Self-regulation of occipital alpha strength is always mediated by learned control of oculomotor processes, whether or not the person is aware of this. He suggests that whatever cognitive strategies are successful in producing alpha changes, they are successful only because they can be efficient mediators of changes in oculomotor processes.

A central result around which Plotkin builds his case in support of his oculomotor hypothesis is the inability of his trainees to significantly enhance alpha in total darkness. This result is interpreted to mean that total darkness is about all that is needed to produce maximum alpha, and that feedback cannot help elevate it beyond this point, whether the feedback is supported by oculomotor, cognitive, or no instructions. This view fails to consider that deficiencies in the training methodology could be involved: not enough training, too frequent interruptions, etc. Moreover, he seems to minimize the fact that his lights-off percent discrimination score for the cognitive instruction group with feedback was at least as great as the comparable score for the oculomotor group. Our comment is that trainees could not exceed their total darkness baselines because their training was deficient. This point is substantiated by the verbal reports. The emphasis on the visual mechanisms, or their absence, in relating the subjective experience to alpha control is, in our experience and that of others, typical of the early phases of training, where effective control is not yet achieved. This is especially true of alpha enhancement.

Experiential Descriptions of the High Alpha State

Plotkin does not believe that there is a relationship between alpha activity and a state of consciousness he describes as "the alpha experience." Some of his statements on this issue are repeated here to show the strength of his emphasis:

1. "There is no direct association between the alpha experience and alpha activity" (p. 94).
2. "Alpha feedback per se is neither necessary for nor especially facilitative of the achievement of the alpha experience" (p. 66).
3. "What we have seen from the research described here is that there is no intrinsic connection between a neurological state of enhanced occipital alpha and a meditative state of consciousness or the state of consciousness referred to in the literature as the 'alpha state' " (pp. 92–93).

It is not unreasonable to fail to find this connection with such brief training so ineffectively supplied. We see acknowledgment being given to the notion that the control of alpha, especially to extreme levels, can be very subtle and difficult to achieve, but this insight is never factored into the interpretation of the subjective report results. Indeed, trainees may be seen as using feedback and/or instructions to overcome the alpha-*depressing* effects of open eyes and light. None of the rationale for this alpha-depressing methodology (helps keep trainees awake, etc.) can obscure the fact that the "subtle" and "difficult" task of training people to extreme levels has not been attempted. Since it has been suggested (Hardt, 1974, Note 3) that the most pronounced aspects of the state of consciousness described as "the alpha experience" do not begin to occur unless and until trainees attain those "extreme levels," it is perhaps not surprising that Plotkin's trainees do not, in general, report experiences like those in the "alpha experience." Furthermore it has been shown that there are individual differences in ability to enhance alpha activity (Hardt, 1974, 1975, in press, Note 3, Note 4; Travis, Kondo, & Knott, 1974b). In Hardt's work, low-ego strength trainees did not do as well at alpha control as did the high-ego strength trainees. Plotkin's research participants were not measured on this important dimension, and so might have been derived primarily from the low-ego strength group, which is inherently less adept at alpha control. This observation should not, however, reduce our concern with the other methodological deficiencies.

It would be helpful in this section to consider the methodological conditions of Plotkin's study from the point of view of the trainee. The condition that would produce the most alpha, and thus provide the trainee with the most opportunity to appraise its subjective qualities, would be the generate trials in the dark. In operant terms the response to be shaped must be in the repertoire of the organisms, and ideally should be maximized by the demand characteristics, at least in early learning. The generate in the dark trials would be the six 3-min trial periods interspersed among two other trial types (one of which requires suppressing). Half of the trainees would not have served as EEG feedback subjects before; they would start their task, having just been given some instructions about keeping a tone loud (but at other times quiet): the peculiar rapid waxing and waning of the tone would be new, fascinating to most, and to some a bit anxiety producing. In the first trials of the session, all efforts by some of the trainees to increase the tone volume (to indicate increased alpha) succeed only in decreasing it. The trials are punctuated with the requirement to change tasks. An impression may start to form that something is being learned, but then the session is over.

As noted, the trainees did not learn to increase their alpha in the dark. As for their subjective evaluations of the experience, there was nothing that could be uniquely ascribed to alpha. What did emerge clearly is what the experimenter led them to expect: Alpha was oculomotor, it was calm and pleasant, or it was nothing, depending on the experimental group to which they had been assigned.

Let us now consider Plotkin's reports of the subjective experiences of three of his groups (FB-OI, FB-CI, and FB-NI) to see if we are led as inexorably to the oculomotor hypothesis of alpha control. In the feedback plus ocular instruction group (FB-OI) only three of the trainees increased above resting levels with light off. This clearly suggests inefficacies in the methodology (too little feedback time, too frequent interruptions and changes of the task, eyes-open training, etc.). Since absolute alpha levels are lower with the lights on, we might expect that the FB-OI group had even lower scores when the lights were on (relative to lights-off resting baselines). Is it any wonder then that "they did not report that the enhance trials were particularly pleasant, serene, or tranquil" (p. 84)? The FB-OI group is identified as the most successful, but remember that the percent discrimination scores obscure absolute levels, and success is reckoned as the recovery of alpha from the sharply depressed levels obtained by eyes-open or eyes-open plus light methodology. The loss of absolute level information would be less misleading if increases or decreases were reckoned from an optimal eyes-closed resting baseline. Such optimal baselines would not be scattered through the training, because that would serve only to distract the trainees. In Kamiya's (1969) chapter in Tart's book of readings, there is a discussion of the problems involved with baselines which interrupt the training:

> With *Ss* who are given only suppression training, their percent alpha time in [the interrupting] baseline conditions tends to decrease. That, is they seem to continue doing what they have been trained to do. But when they have been trained for both enhancement and suppression, they prefer enhancement, and [the interrupting] baselines go up. (p. 496)

If training scores are referred to the nearest interrupting baseline we must note that if baselines change in an upward direction, this will underestimate enhancement control and overestimate suppression control. If the interrupting baselines do not change much, this could be taken as evidence that the trainees are not really "getting into" the really powerful enhancement experience, which spills over into the baselines if it is effectively evoked. If it is not effectively evoked we would expect neither rising (interrupting) baselines nor clear subjective reports of alpha experiences. This is just what Plotkin reports.

The second group to consider is the feedback plus cognitive instructions group (FB-CI). They reported as their most popular strategy the recalling of pleasant experiences. These included that hallmark of the alpha experience: feelings of floating, flying, lightness, or space. And it should be noted that in the lights-off condition this group had the highest percent discrimination score (Figure 2 in Plotkin's article). Had a better enhancement methodology been used, the superiority of this group would have almost certainly been more pronounced, as would their absolute alpha scores and their alpha experiences. Travis, Kondo, and Knott (1974a) have discussed the notion that eyes-open alpha training is primarily concerned with peripheral problems disrupting alpha activity (e.g., oculomotor disruptions), whereas eyes-closed training may be more of a central process. We venture to suggest that central processes are more likely to be determinants of states of consciousness than are peripheral processes involving receptor tuning (eye movements). The cognitive instructions given this group could have helped to overcome the limitations of the too short feedback training. With longer training time the trainees would have discovered for themselves the equivalent of Plotkin's cognitive instructions. Indeed, those instructions were derived from published reports of the discoveries made by earlier alpha enhancement trainees.

The final group to consider is the feedback plus no-instructions group (FB-NI). We are told that their subjective reports "show no *consistent* similarities to experiences that have been widely associated with high and low alpha states" (p. 85). This result is unsurprising. Since the trainees were unaided by instructions and not given sufficient training time to discover consistent control strategies, their alpha changes are primarily due to adaptation and habituation; they probably all did different things, and none of them may have had an alpha experience. It is with great interest that we note Plotkin's report that two of the trainees in this group said "they could increase the tone volume by either very pleasant or very negative feelings" (p. 85). This report provides an opportunity to complete Plotkin's partial view of the influence of arousal or activation level on alpha activity.

Plotkin apparently conceives of a monotonic relationship between alpha and arousal: "occipital alpha is maximal in a given individual when behavioral arousal is maximal . . . and oculomotor arousal minimal" (p. 70). However Malmo (1959) has shown that alpha activity is related to arousal or activation level by an inverted U-shaped function. Low alpha is seen at *both* low and high arousal levels, and high alpha is seen only at an intermediate range of arousal. Malmo's view is useful in understanding why a tense or anxious trainee (high arousal) can increase his alpha activity by relaxing and lowering his arousal level while this same technique may not work for the lethargic or underaroused trainee. Such hypoaroused people can increase their alpha activity by intense emotionality (very pleasant or very negative) which serves to increase the arousal level toward the alpha maximum range. We strongly suspect that Plotkin's two FB-NI trainees who increased tone volume by either very intense positive or negative feelings were of the lethargic, hypoaroused type (*tamasic guna*). It should also be noted that Malmo's inverted U-shaped function may suggest that the adaptation and habituation occurring during the first 2 hr of feedback training may have the function of nudging the trainee into an intermediate range of activation, wherein the naturally occurring alpha is maximal. Perhaps it is only from this naturally elevated alpha activity, where all the alpha-suppressing effects are damped out, that true alpha enhancement can begin. In fact it is just those persons who are described as "stuck" at a given activation level (Roessler, 1973) who have been found least adept at learning enhancement or suppression of alpha activity (Hardt, 1974, 1975, in press, Note 3, Note 4). The attainment of the activation level associated with maximal resting alpha activity involves changes in multiple physiological systems contributing to activation level. Oculomotor functions may be one such contribution, but it is quite bold of Plotkin to suggest that "there is no evidence to date that somatic processes other than oculomotor functions serve as mediators of the self-control of occipital alpha" (p. 71). For example, Kennard and Willner (1945) found that the degree of deep muscle relaxation was related to amount of alpha activity, and Jacobson (1938) found that relaxation training could change the degree of deep muscle relaxation. Van Slyke (1934) showed that the p*H* of the blood, which can influence EEG activity, fluctuates with respiratory phase, and Friedell (1948) indicated that the breathing rate can influence these pH fluctuations. Thus both relaxation and breathing are somatic processes, other than oculomotor functions, which can influence EEG activity. Moreover Flügel (1967) produced increased continuity and definition of the alpha activity of hypoxic patients by treating them with vasoactive agents.

In regard to meditation, certainly we share a social concern with Plotkin to maintain caution about the utility of alpha biofeedback in training meditators. But we strongly believe such experimentation should be encouraged, certainly in terms of formal studies of meditators with tools of psychophysiology and biofeedback. Experimentation should also be encouraged by individuals, practiced meditators, and neophytes alike, on what the EEG alpha rhythm is like in relation to meditation. If some like it and feel it helps them in opening the door to (or in deepening) their meditation, that is fine. If it does not help some, then they need not use it.

From a theoretical point of view there are certainly interesting possibilities for comparing meditation with alpha feedback training. The elevated alpha of Zen and Yoga practitioners is intriguing; one is naturally led to inquire into the similarities and differences between meditation and biofeedback training, not just of alpha, but of the entire psychophysiological spectrum. We therefore believe some of Plotkin's recommendations regarding meditation are of questionable utility: "Alpha feedback would not be recommended as a meditation technique since it would tend to create the self-limiting illusion that our altered states of consciousness are a causal function of our physiology—a physiology that requires an electronic aid for us to alter" (p. 97). We should note in reply that Ram Dass (the former Dr. Richard Alpert) has pointed out that *all* methods can help and *all* methods can be a trap ("self-limiting illusion"). This would apply to meditation too. Meditation is just a method, perhaps a powerful one, but just a method. If you *need* to meditate to induce an altered state of consciousness (to get high) you are trapped no less than if you *need* to apply alpha feedback to induce the altered state of consciousness.

Without taking the time here to develop a comment on the causality relationship between physiology and consciousness, we would nonetheless like to note that a simple signal-and-noise metaphor can encompass both alpha feedback and meditation. The Yogi retires to a quiet forest or mountain where external noise and the resultant internal, sensorily evoked "noise" are reduced. Thus the Yogi can "tune in" the subtle psychophysiological signals of calmness and enhanced alpha activity. The Yogi reduces the noise to detect a weak signal. In the West, with our technology, we have another option: The alpha feedback trainee has the weak signal boosted, technologically, above the noise level and presented, say, in the form of a loud and nonignorable tone. To be sure, the alpha trainee also benefits from reduced environmental stimulation such as bright lights, eyes-open training, frequent interruptions, etc. But the formal equivalence of the tasks of the Yogi and the biofeedback trainee suggests the lack of suitable rationale for the recommendation against alpha feedback as a meditation technique. At the very least, technologically given feedback can show people the possibilities of meditation to which they can graduate after alpha training. Or perhaps they could as well graduate to theta training, or high EEG coherence training. Surely 36 min of alpha enhancement training with less than optimal methodology should not be taken as a prime support for a nonexistence proof of subjective alpha effects, whether or not these effects are similar to meditation experiences which come only after years of dedicated practice.

In sum, we believe that Plotkin has overestimated the importance and influence of oculomotor effects on alpha activity, has undertrained his subjects with less than the best methodology, and has overinterpreted his results, which really only apply to the adaptation and habituation phase of alpha learning. No reevaluation of the alpha experience is warranted by this study.

Hardt (1974) provides a list of eight methodological requirements for producing statistically significant and experientially substantial enhancement of alpha activity in feedback experiments. When studies incorporating these methodological recommendations are conducted, the higher alpha levels thus produced will facilitate the understanding of the alpha experience, control strategies, and the role of the feedback in a way impossible with less efficient training methods.

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Appraising the Ephemeral "Alpha Phenomenon": A Reply to Hardt and Kamiya

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Hardt and Kamiya (1976) present a rather misleading picture of my research (Plotkin, 1976) and of the "state of the art" of alpha feedback research in general. They do this by (a) making numerous assertions of an empirical nature without any empirical data for support and (b) not discussing the relevant research that *is* available (including Kamiya's own earlier work), most of which is inconsistent with their position. In this reply, I shall try to make up for these deficiencies in Hardt and Kamiya's commentary. A brief review of the relevant research and of the nature of the "alpha phenomenon" will make it clear which conclusions are warranted by the available data and what needs to be done to resolve the remaining issues.

Let us begin by considering Hardt and Kamiya's claim that I used "less than optimal methodology." Logically, this assertion would have to be based on either (a) specified research that found significantly higher levels of control than I did, or, better, (b) specified research that independently varied different aspects of the feedback methodology such that the features of my procedure that they criticize were specifically compared with some other methodology, with a finding of higher control scores with the latter. However, we will find that at no point did Hardt and Kamiya offer evidence of either kind. In fact, they offer only their opinions and speculations as to what may constitute an "optimal methodology."

Before we consider the available data, let us briefly review the historical sources of the alpha phenomenon. When my research (Plotkin, 1976) was designed, in the spring of 1973, I was interested in investigating, in a more cautious and well-controlled manner than earlier studies, the phenomenon of alpha control and the associated subjective states. At that time, the alpha phenomenon had come to the attention of the public and the scientific communities chiefly through four research reports—those of Brown (1970), Kamiya (1968, 1969), and Nowlis and Kamiya (1970). It is important to remember in the following discussion that these four reports still constitute practically the only primary sources for the very existence of the alpha phenomenon, and that they were—and still are—the most widely referenced research reports in the area. In light of these facts, I will begin by comparing my methodology with the procedures of these four "classic" studies, which reported both successful alpha enhancement and reliable subjective associations to enhanced alpha.

Hardt and Kamiya have three major criticisms of my methodology: (a) training times were too short, (b) half of the training was with eyes open and lights on, and (c) I switched between enhance, rest, and suppress trials.

First let us consider training length. Although Brown (1970) gave her research participants four sessions of 50-min feedback, she found that the subjective reports she obtained were "relatively consistent over the four practice sessions," so that 50 min of feedback were sufficient to obtain her results. Moreover, Brown's subjects were never able to enhance alpha as high as their prefeedback, eyes-closed baseline periods. Nevertheless, Brown found that the subjective experiences of her participants during enhanced alpha were "uniform," with frequent reports of pleasant thoughts and

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feelings, "dissolved into the environment," floating, etc. Kamiya (1968), who reported that "people can be taught conscious control of their brain activity in a relatively short time" (p. 57), and whose subjects reported states "closely resembling descriptions of Zen and Yoga meditation" (p. 58), appears to have given his subjects only 20 min each of suppression and enhancement training, although this discursive report is unclear. Kamiya (1969) reported an experiment in which his research participants had only 8½ min each of enhancement and suppression training. Nevertheless, most of these persons apparently reported the "high alpha state." Finally, in Nowlis and Kamiya's (1970) study, which found "significant" enhancement of alpha that was associated with "mental states reflecting relaxation, 'letting go,' and pleasant affect" (p. 476), subjects had less than 20 min of total feedback time. *Relative to these four classic studies,* my procedure of giving 72 min of feedback seems quite adequate for research that was specifically designed to determine, as one of its major goals, whether or not the results of these earlier studies would replicate. Moreover, Hardt and Kamiya's claim that significant alpha enhancement and the occurrence of "alpha experiences" require at least 140 min of training directly contradicts the findings of these earlier studies.

Next let us consider the issue of having eyes open and the lights on. Brown (1970, 1971), who found subjective associations of similar kind and strength as Kamiya (1968, 1969), trained all her subjects with eyes *open* and the lights on. (In fact, she used visual feedback.) However, of even greater interest is Nowlis and Kamiya's (1970) study, in which 16 persons were trained with eyes closed and 10 with eyes open (in a "moderately darkened" room): The authors reported no significant differences in subjective reports between these two groups. With these facts in mind, it would seem that my procedure of allowing *each* research participant to attempt alpha control under *both* conditions (i.e., where "looking" is likely—eyes open, lights on—and where "looking" is unlikely—lights off) was the most thorough way of investigating the alpha phenomenon. Considering that Brown (1970) and Nowlis and Kamiya (1970) also trained their subjects with eyes open, one can only be puzzled by the following remark by Hardt and Kamiya concerning my study:

> In a study which had as one of its objectives the assessment of the subjective qualities of enhanced alpha, the requirement of training with eyes open in a lighted room for half of the total training time is surprising. It is rather like asking persons to experience a state which they are prevented from experiencing. (p. 102)

It is peculiar, to say the least, that one of the conditions under which a phenomenon was discovered is now asserted to be a good way of preventing that same phenomenon. Evidently, the alpha phenomenon is a rather ephemeral one indeed.

Finally there is the question of the length and order of trials. My trials were all 3 min long, and I alternated between generate, rest, and suppress trials. Kamiya (1968) employed a similar procedure of alternating between 5-min long enhancement and suppression periods. Kamiya's 1969 procedure was even more like mine: He gave his subjects alternating blocks of 2½-min long enhance, rest, and suppress trials.

To summarize so far, it appears that, relevant to Hardt and Kamiya's criticisms, my methodology was at least as effective as that which was employed by the four studies that discovered the very phenomenon under investigation. Hence we can see that it is certainly not the case that I "designed the feedback training in such a way as to minimize the chances of seeing what [I] did not expect" (p. 102), as Hardt and Kamiya propose. Moreover, there are at least two aspects of my methodology that make it clearly superior to those employed in three of the classic studies (Kamiya, 1968, 1969; Nowlis & Kamiya, 1970). I employed an integrated amplitude measure of alpha strength and a proportional feedback signal, whereas these other studies used a percent time measure of alpha abundance and a binary feedback signal. Travis, Kondo, and Knott (1974) offer data supporting the superiority of the former method, and even Hardt and Kamiya (1975) recommend amplitude integration over the percent time technique.

With the above considerations in mind, it is clear that Hardt and Kamiya's criticisms of my work implicate an even greater criticism of Kamiya's own earlier work, as well as Brown's, which are the very empirical "foundations" of the alpha phenomenon. Thus, if my methodology was too "deficient" to produce "positive achievement at alpha enhancement," then certainly this is also the case for these earlier works. On the other hand, if Hardt and Kamiya's criticisms of my methodology are as incorrect as they are unsupported by data, then my research *was* an adequate test of the phenomenon. Thus, either way, my research has at least demonstrated that the claims of these classic studies—that it was alpha enhancement per se that led to the achievement of alpha experiences—are not accurate. Furthermore, my study necessitates our searching for a new explication of the results of the previous alpha research. The explication that I have offered (Plotkin, 1976; Plotkin & Cohen, in press; Plotkin, Mazer, & Loewy, Note 1) is essentially the same as that offered by several other researchers (Beatty, 1972; Grynal & Jamieson, 1975; Lynch & Paskewitz, 1971; Lynch, Paskewitz, & Orne, 1974; Peper, 1971; Walsh, 1974): The experiences that occur during alpha training may be primarily due to such related variables as suggestion, expectation, initial bias, or the demand characteristics of the experimental setting, as well as to the personal characteristics of those persons who engage in such training. We must conclude that Hardt and Kamiya must also now explain the results of the classic alpha studies in terms of such variables as suggestion or persuasion. This state of affairs is particularly ironic in light of one of Kamiya's (1968) conclusions from his earlier alpha research:

> Perhaps our increasing concern over control of the individual by psychological persuasion could be diminished. People with full control of their internal states might be better prepared to resist external control. (p. 60)

There are quite a few research reports that have failed to replicate the alpha phenomenon —both the conclusions of alpha enhancement above optimal baselines, and of reliably associated subjective states (e.g., Beatty, 1972; Lynch et al., 1974; Orne & Paskewitz, 1974; Peper, 1971; Plotkin, 1976; Plotkin & Cohen, in press; Regestein, Pegram, Cook, & Bradley, 1974; Plotkin et al., Note 1). Moreover, I have been unable to find *any* well-controlled research that *has* replicated the phenomena. The only such works that Hardt and Kamiya refer to are four of their own papers (Hardt, 1974, Note 2, Note 3; Hardt & Kamiya, Note 4), none of which are as yet published or generally available. If they ever *do* become available, if they *do* claim that they have shown alpha enhancement significantly above eyes-closed resting levels, and if they *do* claim that the alpha experience is reliably and intrinsically associated with these higher alpha levels, we will have to ask the following two crucial questions about their research: (a) Were adequate procedures used to demonstrate actual increases above optimal eyes-closed levels, and (b) were the necessary experimental controls adequately employed to determine to what degree the reports of alpha experiences were explanable in terms of the aspects of the feedback *situation* other than alpha levels per se. Unless we are able to confidently answer yes to both these questions, Hardt and Kamiya's recent work will not further our understanding of the alpha phenomenon. Let us now consider the recent research relevant to the above two questions.

First I will review the literature relevant to the question of whether or not alpha can be enhanced significantly above optimal eyes-closed baselines. Lynch and Paskewitz (1971), in a review of the literature up to that time, concluded that

> the alpha densities which can occur in the feedback situation may approach those seen in that same individual under optimal baseline conditions, but will not significantly exceed them. (p. 213)

These authors hypothesize that feedback will allow persons to increase their alpha levels *only* in circumstances that cause an initial decrease below optimal baselines (i.e., "adaptation and habituation"). Podlesny and Raskin (1974) gave two of their subjects 120 min of enhancement feedback (with eyes closed) over six sessions. They found that training did not produce alpha densities or alpha durations greater than eyes-closed resting levels. Travis et al. (1974), although giving their subjects only 50 min of enhancement feedback with eyes closed, found that those persons who received proportional feedback increased their integrated alpha levels above initial baselines. However these increases above baseline were very small and seemed to level off after 20 min. Moreover, the prefeedback baseline period was only 2 min long, which may very well be too short to obtain an accurate baseline index, since initial baselines will often rise spontaneously for several minutes. Paskewitz and Orne (1973) reported that with eyes open in a totally dark room, their participants approached asymptotic density within 2 min without *any* feedback. Furthermore, with 150 min of enhancement feedback (over six sessions), these persons never reached their preexperimental eyes-closed baseline levels (averaged over a 3-min period). However, we should note that Paskewitz and Orne used a percent time measure, which is not as sensitive or effective as integrated amplitude, as discussed above. Finally, Regestein et al. (1974), also using a percent time measure, found no striking increases in alpha density over 4- and 12-hr sessions of continuous enhancement feedback with eyes closed. Moreover, their research participants, who were paid for successful alpha production, found the experience to be generally "boring."

Although these few reports suggest that even extended training with eyes closed does not lead to significant increases above eyes-closed baselines, none of them may, in fact, constitute an adequate test since none employed an integrated amplitude measure with long training. Hence, we must conclude that, relative to Hardt and Kamiya's commentary, there are absolutely no published studies in the literature (including the four classic studies) that have adequately tested the possibility of "positive achievement at alpha enhancement." Thus, it *is* possible that longer training (with eyes closed, an integrated amplitude measure, and proportional feedback) *will* result in the enhancement of alpha strength above eyes-closed baselines, although it should be remembered that such a demonstration would constitute a different finding than that previously reported as the alpha phenomenon. However, even if this is the case, researchers will have to be most careful in obtaining and interpreting their participants' subjective associations. For, as we have seen, Hardt and Kamiya must now account for the occurrence of alpha experiences in all previous studies to be due to variables other than "positive achievement at alpha enhancement," which they now hold could not have been achieved with the inadequate methodologies that all previous studies have employed. And since these situational variables were so potent in the past, it is clearly mandatory that their effects are carefully controlled for in the future.

There appear to be several aspects of the feedback situation that could very well account for most or all aspects of the subjective experiences reported to be associated with alpha. Chief among these are (a) suggestion and expectation, (b) the sensory-deprivation qualities of the feedback circumstances, and (c) the experience of success at the feedback task. Let us consider these individually.

It is well known that suggestion or expectation has a very powerful influence on the often unusual experiences associated with hypnosis and psychedelic drugs, as well as on all sorts of more common experiences. I have already listed the many papers whose authors have hypothesized that suggestion and expectation—explicit or implicit, conscious or unconscious—are likely to play important roles in the development of alpha experiences during alpha feedback training. Walsh (1974) and Plotkin (1976) have offered evidence to this effect.

Moreover, several studies (Orne & Paskewitz, 1974; Peper & Mulholland, 1970; Plotkin & Cohen, in press; Travis, Kondo, & Knott, Note 5; Yock, Note 6), which have minimized implicit or explicit suggestion, have failed to find a reliable association between enhanced alpha and the alpha experience, although Hardt and Kamiya would argue that this is due to inadequate training procedures.

Another significant characteristic of the typical feedback situation (and of the one that Hardt and Kamiya strongly recommend) is the way in which it resembles a sensory-deprivation experiment. Trainees are usually asked to sit in a comfortable chair, or to lie on a bed, which is typically situated in a soundproof room with low lighting or none at all. Moreover, trainees are often asked to close their eyes, relax, and not to move around (in order not to disturb the electrodes). On top of all of this, the standard feedback signal is a monotonous tone, often appearing over a headphone set, which the trainee is constantly monitoring in order to track his changing alpha levels. This last feature also reminds us very much of many meditation techniques, in which the meditator focuses or limits his attention to a monotonous, repetitive process, or to an unchanging object, such as one's breathing, a mantra, chanting, or a mandala. Considering these aspects of the typical feedback situation, we would be surprised if the trainee did not report that he was relaxed, and we would certainly have no grounds for amazement if we heard reports, similar to sensory-deprivation effects, of loss of body awareness and the associated feelings of lightness, floating, or space, to say nothing of "loosing awareness of the external environment." However, as with sensory deprivation, some persons may react to this feedback situation with anxiety or panic. This is supported by the reports of a few of my research participants who felt that high alpha was associated with anxiety (Plotkin, 1976), and by Orne and Paskewitz's (1974) observation that their subjects' feedback-enhanced alpha levels did not decrease during periods of reported anxiety and heightened arousal produced by the anticipation of electric shock.

Finally, let us consider the possible effects of the experience of success at the feedback task. This factor could be helpful in explaining the pleasant affect and enthusiasm of the subjective reports. For many persons, the self-control of an "involuntary" bodily process, especially one as mysterious and vital as brain wave activity, is certainly not an ability that is thought to be trivial or easily learned. Hence, the acquisition of such a skill can be justifiable cause for feelings of unusual self-mastery and the accompanying positive affect and elation. Lynch et al. (1974) report that their subjects' "positive subjective reactions to the feedback procedures" were most likely due to "the fact that subjects were told that the experience would be a pleasant one, and all subjects had some degree of success in increasing the amount of feedback they received" (p. 408). We may also note in this context that the "placebo" characteristics of successful feedback training may very well be significantly more powerful than those of drug placebos. A trainee would be expected to become much more involved in—and thereby influenced by—changes that he can see *himself* as having produced than by changes that he attributes to drug effects (even though he is also, in fact, the agent of change in the latter case when the drug is an inactive placebo). Thus, we would expect the experience of success in electroencephalogram (EEG) feedback training to greatly amplify the effects of any suggestions or expectations that may already be active.

Having considered the above three categories of variables, we are now in a better position to critically appraise the assertion, by Hardt and Kamiya or others, that the alpha experience is reliably and intrinsically associated with enhanced alpha levels per se produced by longer training with eyes closed. First, let us be clear that this assertion *may*, in fact, be correct. However, we can now understand that any alleged demonstration to that effect would be meaningless if it did not adequately control for the effects of these other variables, especially when we consider that all three of these variables would be expected to increase in potency with the increases in training length that Hardt and Kamiya claim to be necessary to show "positive achievement at alpha enhancement." (In fact, it is quite possible that Hardt and Kamiya's observation of more frequent and "powerful" alpha experiences with longer training will be found to be primarily due to the increase in the power of these three variables over longer training than due to increased alpha strength, if any.)

I would like now to consider some further issues that Hardt and Kamiya have raised.

First, I have three comments about their criticism of my electrode placement (occipital–frontal). (a) Nowlis and Kamiya (1970) used the same occipital–frontal placement with half of their subjects and an occipital–central placement with the other half. They reported that "there was no evidence that [the different electrode placements] had any influence on success at the task" (p. 481). Moreover, they reported no differences in subjective reports when using these two placements, nor were there any striking differences between this report and that of Kamiya's (1969) study, which used an "occipital to earlobe" placement. (b) Other researchers have used other placements (e.g., O_z–ear, O_1–O_2) but no one has reported any differences in results, relevant to the type of research considered here, which hinges on electrode placement as long as at least one of the two active electrodes is over the occiput. (c) Hardt and Kamiya's comment that "the use of a frontal site (near the eyes) considerably increases the oculomotor disruption of any recorded alpha activity" (p. 100) is not relevant to my research for two reasons. First, although I did not mention it in the apparatus section due to a shortage of space, I did at all times monitor the unfiltered EEG by means of a Grass EEG amplifier (Model 7P511) and strip chart recorder. In addition, the input to the digital quantifier (from the AT-1) was recorded (in blips) on the Grass channel directly beneath the EEG recording. Artifacts from muscle tension or eye movements were very rarely seen, but on the few occassions on which they were observed the graphic recording allowed us to correct for their artifactual effects on alpha scores, and to warn the research participant to avoid muscle tension as a control strategy. Second, the F_4 placement is evidently sufficiently far back on the scalp to avoid the large eye artifacts that Hardt and Kamiya mentioned.

Next, let us consider Hardt and Kamiya's comment that "by frequently interrupting the trainees (every 3 min) to change the task, it is guaranteed that the subtle psychoneural state of high alpha will not have much time to develop and will probably not be seen" (p. 102). As is true for most of their assertions, Hardt and Kamiya have offered no data to support this claim, nor the related claim that frequent baselines during training "serve only to distract the trainees." Furthermore, these claims are most puzzling in light of the data that *are* available that show that frequently changing the task does not guarantee the nonoccurrence of the alpha experience. These data are found in Kamiya's own earlier work, in which the "psychoneural state of high alpha" was first reported. As we saw earlier, Kamiya (1968, 1969) altered the experimental task in nearly the same way that I did. Moreover, I could argue that giving the trainee an opportunity to alternately practice alpha enhancement and suppression with interspersed rest periods leads more quickly to "effective" alpha enhancement than enhancement training alone, since the trainee learns the nature of the control process quicker and more thoroughly when he understands the dynamics of suppression as well as enhancement. Furthermore, rest periods supply the opportunity for the trainee to contemplate his degree of success and strategies up to that point, and to "regroup" while not under the pressure to perform. Although this argument seems to be at least as compelling as Hardt and Kamiya's, it would clearly be inappropriate to state it as an implicitly proven finding until the relevant data are available.

Third, Hardt and Kamiya criticized my baseline-measuring procedure as being inappropriate since it obscured "any carry-over effects from all the previous training" (p. 103). However, a glance at the data demonstrates that this was not the case: Baseline periods remained relatively stable throughout the sessions, showing only the expected small rise (158, 159, 163, 166, 167, 164). Since the baselines showed virtually no change relative to the change across trials of the suppression and enhancement scores, it is not the case that carry-over effects were obscured. Moreover, I did not "assume" that this would be the case. As mentioned in my article, pilot work was conducted before an effective baseline-measuring procedure was decided upon. This pilot work, as well as the research itself, demonstrated that persons *were* able to quickly gain sufficient control over alpha to "not do it" during the baselines. Hardt and Kamiya went on to assert that my baseline procedure would not be effective if I was measuring breathing rates while "climbing up or down a 10,000-ft. mountain" (p. 103). Although this observation is undoubtedly correct, it clearly has no relevance here—turning alpha on and off is simply not a similar task to mountain climbing. Moreover, the relatively high stability of my baselines attests that I did not make unreasonable demands on the baseline situation. Once again, Hardt and Kamiya should be encouraged to first gather the necessary data before making assertions of an empirical nature. Finally, in this connection, Hardt and Kamiya have stated that I gave no reasons for the insufficiency of Kamiya's (1969) interpretation of *his* rising baselines. I assumed that the insufficiency would have been clear in the context of my discussion: Namely, that no data were presented which ruled out the equally obvious interpretation—that both the baseline and enhance levels were showing spontaneous increases independent of training effects per se.

There is another issue related to baseline measuring that Hardt and Kamiya bring up. They assert that if my baselines did rise, my success scores would underestimate enhancement and overestimate suppression. On the other hand, if they did not change much, "this could be taken as evidence that the trainees are not really 'getting into' the really powerful enhancement experience, which spills over into the baselines if it is effectively evoked" (p. 105). Thus, with no adequate data in hand, Hardt and Kamiya are asserting that, regardless of my results, my methodology *must* have been inappropriate. However, there is no evidence that alpha enhancement must "spill over into the baselines" or even that it usually does. Moreover, those persons in my feedback groups who reported experiences most like the alpha experience did not show baselines rising any faster than the average, nor were their baselines following enhance trials any higher, on the average, than their baselines following suppress trials.

Fourth, Hardt and Kamiya have criticized my success scores for allegedly obscuring "the 'recovery' aspects of the training by eliminating absolute magnitudes of alpha activity" (p. 102). By "recovery aspects," they refer to the recovery up to eyes–closed resting levels from the initial depression caused by opened eyes, lights, or other factors. I have two comments: (a) As we have seen above, there are absolutely no published studies that report alpha enhancement significantly beyond these "recovery" levels. Hence it is a rather empty criticism that I may have obscured an effect that always occurs. (b) There are, as yet, no widely agreed upon ways of measuring optimal eyes-closed baselines, and thus no agreed upon ways of measuring (or obscuring) "recovery" effects.

A final "methodological deficiency" that Hardt and Kamiya find in my study is my failure to measure the "ego strength" of my trainees. Due to this oversight on my part, Hardt and Kamiya caution us that my trainees "might have been derived primarily from the low-ego strength group, which is inherently less adept at alpha control" (p. 104). With this criticism, Hardt and Kamiya reach new heights of post hoc speculation. Clearly, the burden of proof rests squarely on Hardt and Kamiya's shoulders when they make this and their numerous other unsupported criticisms. Moreover, it does not appear that my research participants were any less successful at alpha enhancement than anyone else's. As reported in my article, 46% of my feedback trainees were successful in enhancing their alpha strength significantly above their baseline levels with the lights off. I know of no study that has shown greater success at enhancement than this under lights-off conditions. Hardt and Kamiya repeatedly overlook this fact in their references to my "ineffective," "deficient," and "less than optimal" methodology.

In their commentary, Hardt and Kamiya remark in two places that I have missed an alternative interpretation of my percent discrimination scores in my Figure 2. They note that with lights off, the feedback–Cognitive instruction group did better than the feedback–Oculomotor instruction group, and that the no feedback–Cognitive instruction group scored higher than the no feedback–Oculomotor instruction group. However, if we glance at this figure we can see that the differences between these two pairs of groups are extremely small, and given the relative size of the other clearly marked insignificant differences on this figure, I find it hard to understand how anyone would want to offer an interpretation of differences of such magnitudes. Two post hoc comparisons show that Hardt and Kamiya have based their "alternative interpretations" on an F of .188 ($df = 1, 16, p > .6$) and on an F of .057 ($df = 1, 16, p > .8$), respectively.

To summarize so far, although alpha levels significantly beyond optimal eyes-closed baselines *may* be possible, no one has yet demonstrated them. Moreover, based on the available data, we must conclude that Hardt and Kamiya have not demonstrated that my methodology was "deficient" from the point of view of the trainee, the obtained enhancement scores, the previous research, or general methodological considerations. In addition, we have seen that, even if higher alpha levels can be achieved with longer training, carefully controlled research will be necessary to demonstrate that the alpha experience is in any way *intrinsically* associated with enhanced alpha per se.

I would like now to consider a few remaining points that Hardt and Kamiya have raised about my design and conclusions.

Toward the beginning of their commentary, Hardt and Kamiya demonstrate some confusion about the nature and significance of the two control strategies that I investigated—the Oculomotor and Cognitive strategies. To their understanding, the Oculomotor strategy "does not imply any necessary behavioral or psychological strategies" (p. 101). Rather, they think it merely concerns "a hypothesis about how training of alpha control is made possible physiologically" (p. 101). This is clearly a misunderstanding. First, it should be clear that all strategies, regardless of their nature, (a) have a cognitive aspect and (b) are behavioral (i.e., not merely physiological). This is simply a logical reminder that to speak of a "strategy" is to say that certain distinctions are being made and acted upon (even if unknowingly); therefore, all strategies have a cognitive aspect and are behavioral, since to use the term *cognitive* is usually to speak of the act of making distinctions or of the aspect of *behavior* that involves distinction making. Thus, to say that a given strategy is not cognitive is tantamount to saying that it is not, in fact, a strategy at all. It is for this reason that it was unfortunate that I chose *Cognitive* for the label of one of the strategies since it may have implied to some that the other (Oculomotor) strategy did not have a cognitive aspect (or that it was not behavioral). The Oculomotor strategy is, of course, behavioral and it does have a cognitive aspect—namely, the trainee distinguishes the effects on the feedback signal of different sorts of changes in his or her "looking" behavior. Moreover, the Oculomotor strategy also has a physiological (or visible performance) aspect—namely, that changes in the oculomotor system have been shown to be concomitant with occipital alpha changes and alterations in looking behavior. (Hence, we can see that it is certainly not my belief that "if it is physiological, no cognitive processes could be involved" (p. 101), as Hardt and Kamiya fear). The Cognitive strategy was given that label simply because it does not have a visible physiological aspect (other than occipital alpha itself), and thus is more "purely" cognitive. However, I acknowledge the confusion that I may have generated with this choice of language.

Second, Hardt and Kamiya's comments express a related misunderstanding of Mulholland and Peper's work on occipital alpha and the oculomotor system (e.g., Mulholland, 1968; Mulholland & Peper, 1971). They imply that by having based parts of my experimental design and conclusions on the work of Mulholland and Peper, I did not stay on the behavioral level. However Mulholland and Peper's work (as reviewed in my article) *does* primarily involve the behavioral level. The major paradigm they employed in this aspect of their work was to ask their research participants to engage in various forms of looking behavior, such as focused or blurred tracking, while they recorded changes in the occipital alpha rhythm. Their results demonstrate reliable and systematic correlations between consciously initiated visual control processes and occipital alpha densities. This research directly demonstrates the effectiveness of the Oculomotor strategy of alpha control on the behavioral level. However, it is precisely because one cannot conclude from this research that the Oculomotor strategy is the *only* effective control strategy, that studies of the kind I conducted are necessary. However, the results of my study demonstrated that (a) the Cognitive strategy that I studied was not effective for most persons, but that (b) there was a different "cognitive" strategy (involving the degree of sensory awareness) that *was* effective for nearly all persons, and (c) this latter strategy is *not* independent of the Oculomotor strategy, as shown by the fact that those persons who employed this strategy were handicapped to the same degree by the same conditions that handicapped the persons employing the Oculomotor strategy (i.e., no lights). Moreover, given that the same changes in looking behavior that constitute the Oculomotor Strategy also occur (incidentally) during exercise of this "cognitive" strategy, it is a reasonable and parsimonious hypothesis that the same physiological processes occur during both strategies. In addition, a search of the literature concerning the occipital alpha rhythm does not reveal any experiment in which oculomotor functions have been excluded as a cause of alpha strength changes.

It is in this connection that Hardt and Kamiya have misunderstood the significance of the findings of Chapman, Cavonius, and Ernest (1971), who reported normal alpha activity in eyeless subjects. A reading of the research on occipital alpha and the oculomotor system (e.g., Mulholland, 1968, 1973; Peper, 1971; Wertheim, 1974) makes it clear that it is not hypothesized that occipital alpha blocking is caused by—or directly indicates—*peripheral* oculomotor activity (i.e., activation of the muscles in and around the eye), but rather that it indicates *central* (cortical) oculomotor processing (i.e., the neural efferent activity involved in oculomotor activation.) For example, Mulholland (1973) hypothesized that it is "both efferent visual control processes and afferent processes and their integration occurring in the cortex [that] are involved in the suppression of alpha" (p. 140). Chapman et al.'s research was designed to refute charges that occipital alpha activity was an artifact related to eye activity per se. It was not a disproof of the association between occipital alpha and oculomotor processing in the cortex. Thus, although the peripheral physiology (i.e., the eyeball and extraocular muscles) is most useful in studying the oculomotor system, this physiology is not hypothesized to be the site or source of alpha blocking. Moreover, the finding of alpha blocking in eyeless subjects to "hard" mental tasks is precisely what we would expect. This is the same phenomenon seen in normals, and has been explained as a function of cortical oculomotor activation as a normal component of the well-known orienting response (Mulholland, 1968; Wertheim, 1974; Zickmund, 1969). This continued oculomotor efference in eyeless persons may be to some extent analogous to the "phantom limb" phenomenon in amputees.

At another point, Hardt and Kamiya attempt to refute the Oculomotor hypothesis by claiming that subjects' emphasis on visual mechanisms for alpha enhancement is typical only of the "early phases of training." However, this finding is quite consistent with the oculomotor hypothesis. It would be expected that trainees would no longer mention visual strategies after a little enhancement training simply because "looking" is what they are *not* doing, and hence it is natural that they would instead report what they *are* doing (e.g., daydreaming, becoming anxious, meditating, problem solving, being bored), even though the significance of what they are doing, relative to alpha enhancement, may only be that they are not "looking." This line of reasoning may also explain why reports of visual strategies persist in alpha suppression: In this case, "looking" is what the trainee *is* doing.

In this connection, we can see that the strategy reports of alpha-enhancement trainees may merely tell us what kind of nonsensory state different kinds of persons are most disposed to enter while reaching high degrees of oculomotor inactivity in the context of feedback training. It is important to note, however, that the phenomenological authenticity of the alpha experience is in no way being called into question here. The question is whether or not alpha enhancement per se is at all instrumental in—or intrinsic to—the achievement of this experience. Moreover, it is not the case that I do "not believe that there is a relationship between alpha activity and . . . 'the alpha experience'" (pp. 103–104), as Hardt and Kamiya claim. What I have repeatedly suggested is that there is no *intrinsic* or *direct* relationship between the two. They *are* associated in the sense that the "alpha experience" is one instance of a very wide *range* of states that, as a class, are reliably *accompanied* by enhanced alpha.

Hardt and Kamiya thought it "quite bold" of me to suggest that there was no evidence that somatic processes other than oculomotor functions serve as mediators of the self-control of occipital alpha. They then listed a few studies that seemed to them to offer such evidence. However, the finding that some somatic processes sometimes change concomitant with alpha changes does not imply that these processes necessarily do—or even can—serve as mediators of alpha control. The first step of such a demonstration of mediation is to show that these processes change *while* a person is controlling alpha. The available research, reviewed in my article, shows negative findings whenever such investigations have been conducted.

Concerning the connection between arousal level and alpha activity, Hardt and Kamiya have once more demonstrated their unfamiliarity with the recent research, for they have deemed it necessary "to complete [my] partial view of the influence of arousal . . . on alpha" (p. 106) by referring to the very theory (Lindsley, 1956; Malmo, 1959) that I sought to amend based on more recent, conflicting evidence (e.g., Mulholland, 1968; 1973; Orne & Paskewitz, 1974). The earlier theory claimed that high alpha is seen *only* at an intermediate arousal level. Although this is often the case, more recent research (e.g., Mulholland, 1968, 1973) has suggested that the desynchronization of occipital alpha that is thought to be intrinsically associated with "hyperarousal" is in fact a function of the cortical oculomotor activity that often occurs along with heightened arousal. However, when oculomotor activation does *not* accompany hyperarousal, neither does EEG desynchronization. Orne and Paskewitz (1974) have recently found, for example, that

> anticipation of electric shock did not depress alpha activity in a feedback situation though it was associated with reported anxiety and heightened arousal indexed by greatly increased heart rate and number of spontaneous skin conductance responses. (p. 458).

As for the utility of alpha biofeedback, this cannot be determined through "formal studies of meditators with . . . biofeedback," as Hardt and Kamiya suggest. If a meditator enhances alpha and/ or achieves a meditative state while meditating during alpha feedback, we have learned nothing about the potential effects of feedback-augmented alpha levels per se, nor anything about the intrinsic subjective correlates of enhanced alpha, since the meditation and biofeedback variables are totally confounded. The *mere* fact that both meditators and neophyte feedback trainees may increase their alpha strength tells us nothing *else* about the potential similarities of the two activities. It would be most instructive, in fact, to ask a meditator to enhance alpha (with feedback) with*out* entering a meditative state. Moreover, it appears that the connection between meditation and enhanced alpha levels may not be very robust, in the first place. Travis, Kondo, and Knott (1975) have recently found that the occipital alpha production of Transcendental Meditators did not change significantly during their meditation, and did not increase over premeditation baselines, as previously reported by Wallace (1970).

My recommendation against using alpha feedback as a meditation technique was based on my finding that enhanced alpha levels per se were not reliably or intrinsically associated with a meditative state. If adequately controlled studies of longer training, which we are now conducting, reach the same conclusion, then I would maintain my recommendation despite Hardt and Kamiya's reminder that "all methods can be a trap," simply because it will then have been verified that alpha enhancement per se is not a method at all. On the other hand, if these studies do show the hypothesized association between alpha and meditation, then alpha feedback would, of course, be useful for certain persons. However, in this case, the utility of alpha feedback would be in the *behaviors* that the trainee learns he must engage in in order to alter the feedback signal in the desired manner, and not in the mere fact of enhanced alpha strength. Moreover, there would be, in any case, a significant difference between the "methods" of meditation and alpha feedback: in the former case, there are no *external* aids.

Finally, I would like to make a few brief remarks about a basic assumption that underlies the attempt to "map" subjective states with the EEG and other physiological measures. Implicit in this endeavor, for many researchers, is a Cartesian mind–body dualism (Grossberg, 1972), in which physiological, and especially neurophysiological, states of affairs are thought to be identical with—or the cause of—behaviors, experiences, or states of consciousness. This logical error, explicated by Ossorio (1974), Ryle (1949), and Wittgenstein (1958) often results in the use of confusing and misleading phrases such as "psychoneural state" or "psychophysiological signals of calmness and enhanced alpha activity," as found in Hardt and Kamiya's commentary. In another paper (Plotkin & Schwartz, Note 7), we explore in more detail in what ways the notion that physiological states are formally equivalent to states of awareness constitutes a serious logical impediment to the understanding of persons, behavior, and consciousness.

In conclusion, considering the available data, it is at best questionable at this point that "technologically given feedback can show people the possibilities of meditation" (p. 107) or that there is a "formal equivalence of the tasks of the Yogi and the biofeedback trainee" (p. 107), as Hardt and Kamiya conclude. What the *scientific* community requires now is responsible research rather than exorbitant claims and enthusiastic speculation.

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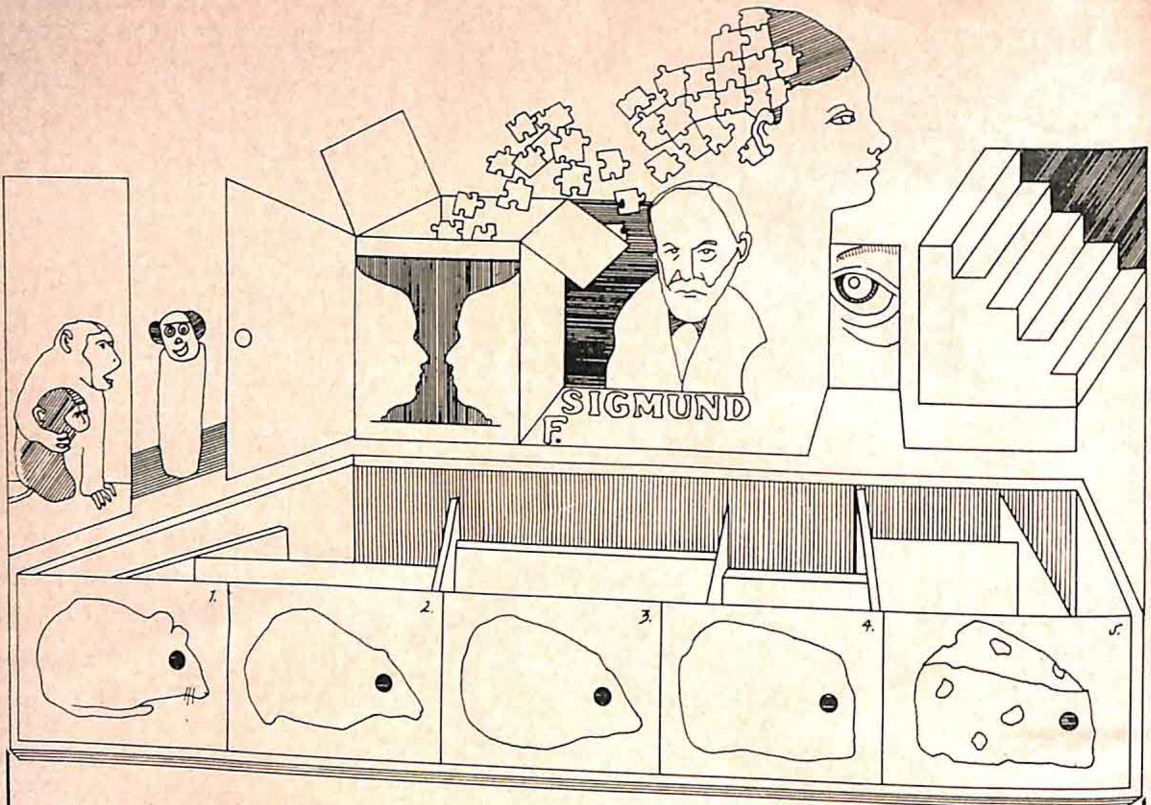

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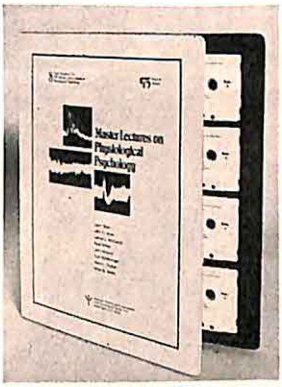

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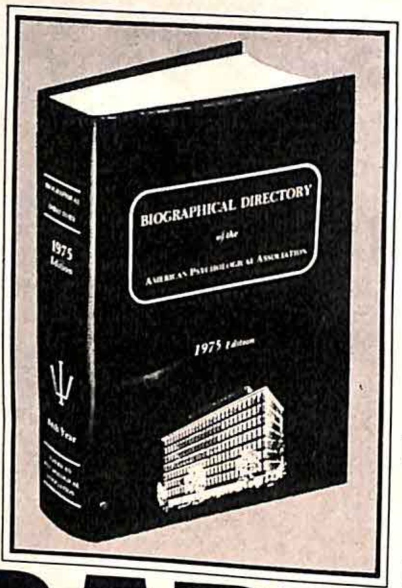

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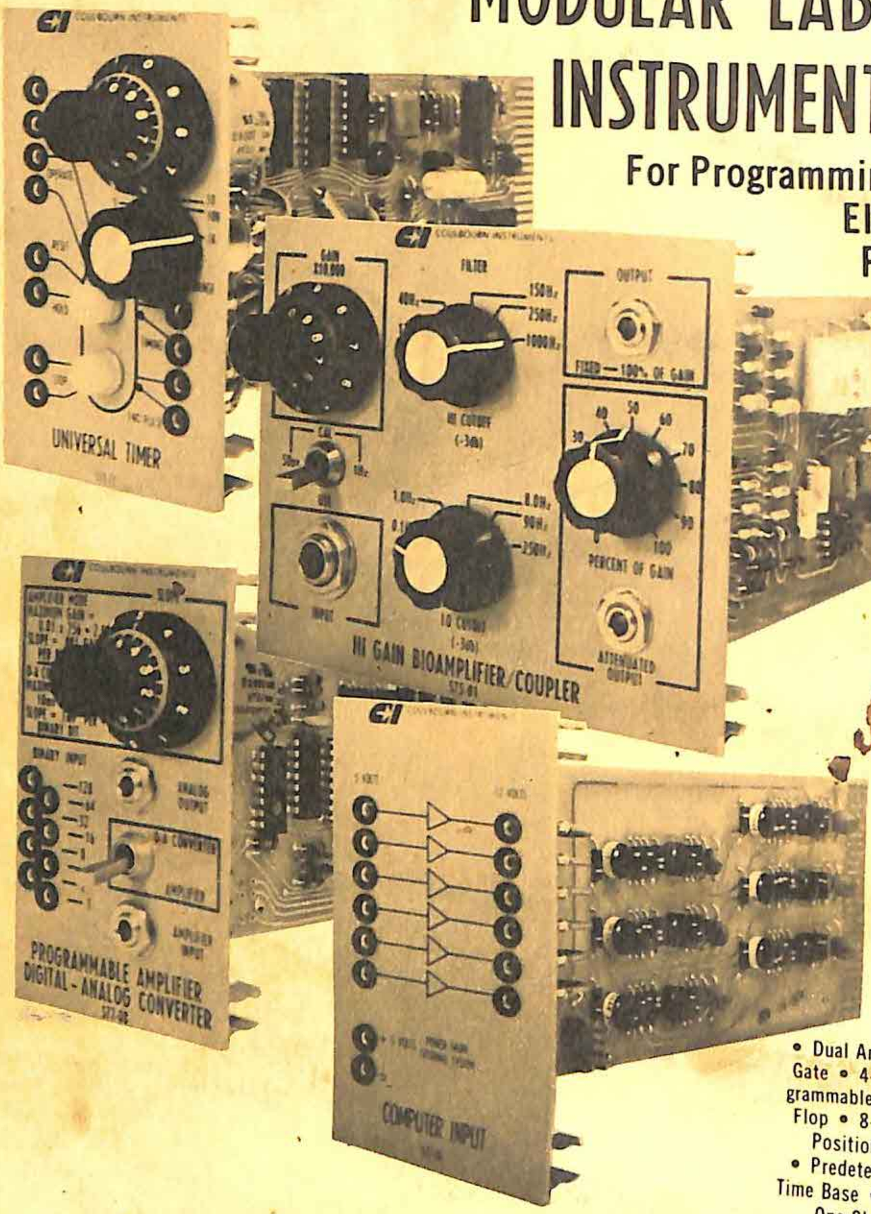

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